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EDUCATION FOR LIVING

THE Central Advisory Council for England was appointed by the Minister of Education under Section 4 of the Education Act, 1944, in December of that year. By the terms of the Education Act it was decreed that "there shall be two Central Advisory Councils for Education, one for England and the other for Wales and Monmouthshire, and it shall be the duty of these Councils to advise the Minister upon such matters connected with educational theory and practice as they think fit, and upon any question referred to them by him. . . ." By agreement with Mr. R. A. Butler, who was Minister of Education in December 1944 and in the early months of 1945, the Central Advisory Council for England decided, as its first task, to examine the content and methods of education in those schools from which "the actual transition from life at school to independent life" is made; and the results of this preliminary inquiry have recently been published in the book entitled "School to Life".*

Since one of the most important and urgent tasks of educationists is to combine fixity of aim with diversity in application, the inescapable question is how general or liberal, as distinct from specialist or vocational, education can be so adapted to differing abilities and aptitudes that, while appealing deeply to each, it can remain in goal and essential outlook the same for all. The larger part of the question is how general education can be so adapted for the great majority, who are possessed of a lower facility with ideas than the intellectually gifted minority, that it can implant the power of thought and expression, the sense of relevance and value. It is, indeed, easy enough to postulate that, if general education is to be adapted to meet the needs of young people with differing abilities and aptitudes, it must make increasing use of what appeals directly to the senses. But merely to make this adjustment will not alone suffice. Those whom ideas leave unmoved must come to know them in more specific forms—mathematics when it is related to some mechanical task, history when it touches some trade, and design when it is wedded to some manufacturing process. Or, again, although some learn of democracy and of its privileges and responsibilities by growing familiar with ideas, all—and more especially the less intellectually gifted—learn also through action and by example; and, for those who see life primarily as action, the adaptation of general education must be such that they are roused to a realization of the connexion between ideas and day-to-day action.

Whatever detailed criticisms there may be of "School to Life", it will be generally agreed that the Central Advisory Council has done well to recognize the urgency of the need for this adaptation. In one striking passage in the report it is categorically declared: "The system is already established of transferring children for the post-primary years of school to some form of secondary school according to their different aptitudes. While the traditional curriculum and methods of the older endowed schools

*H.M. Stationery Office. 2s. 6d.

may continue to meet the needs of many, the majority of children will be attending secondary schools which do not belong to this tradition. . . . Industry may well suggest something both of subject and method which will help rather than hinder their efforts."

It is, however, to be noted that, in citing examples of some of the ways in which the practical element can be introduced to general education at the secondary stage, the Council is insistent that "the danger to be avoided is that of subordinating the whole personality to a narrow conception of industry's need"; and not the least valuable feature of this report is, in consequence, an insistence on the distinction between general education with a technical bias and vocational training. Although the insistence on this distinction has led the Council to accept such questionable conclusions as "works schools are no substitute for county colleges" and "industry should provide its own special training", it has resulted in a balanced appreciation and criticism of education as "a preparation for a useful and satisfying life".

Even if, as has been suggested, the Council has misunderstood its function in addressing this report to a wider audience than was the practice of the Consultative Committee in framing the Hadow and Spens Reports, it has succeeded in focusing attention on the difficulties and uncertainties which have, in the past, needlessly attended "the transition from school to independent life". In endorsing the view, expressed fifty years ago by the Bryce Commission, that secondary education is technical because "it teaches the boy so to apply the principles he is learning, and so to learn the principles by applying them, or so to use the instruments he is being made to know, as to perform and produce something, interpret a literature or a science, make a picture or a book, practice a plastic or manual art, convince a jury or persuade a senate, translate or annotate an author, dye wool, weave cloth, design or construct a machine, navigate a ship, or command an army", the Council is saying nothing that is new. In emphasizing the further conclusion of the Bryce Commission that "technical education is secondary" to that part of education which, in the words of the report of the Harvard Committee on "General Education in a Free Society", looks first to the needs of a young person "as a responsible human being and citizen", it is underlining the most important—and the most neglected—conclusion of the Percy Committee. If there is to-day no need to argue that "the position of Great Britain as a leading industrial nation is being endangered by a failure to secure the fullest application of science to industry", it is, in present circumstances, still essential to subject to the most ruthless analysis the further conclusion that "this failure is partly due to deficiencies in education".

The deficiencies to which the Percy Committee referred are not to be explained solely—or, indeed, satisfactorily—by comparing the provision for technical and other forms of secondary education, or by deploring the number of university graduates in relation to the rest of the population. Those educationists and politicians who imagine that merely to increase the number of those who are given specialist education, whether at the secondary or the university level, is to safeguard the position of Great Britain as a leading industrial nation would do well to ponder the warning in a recent editorial comment of *The Economist* that "there are plenty of managers in this country who cannot manage, teachers who cannot teach, statisticians who cannot count, and women who cannot wash up. The country, in fact, abounds in incompetents. What, however, does not seem to be realized is that this incompetence is not due to lack of training, but reflects the failings of the country's educational system. The remedy is not to give a professional smear to a bad construction, but to secure what is so often overlooked—the training of minds."

The continued failure to appreciate this fundamental need has arisen, in part at least, from the widespread neglect of the dictum that "you may not divide the seamless coat of learning". In the weaving of it there are three strands—accurate knowledge, a wise competence, and a true valuation—and, until each is conceived as an essential and inseparable part of total education, whatever bias it may be given, so long will educationists fail to fulfil their primary responsibility. The true purpose of general education is not to teach the individual many things, or even much, but to afford him the opportunity to 'learn to think' and to 'learn to learn'. To those who realize that, because its purpose is not thus conceived by many who either legislate or teach, there is the further failure to appreciate that technical education can afford a unique opportunity to translate into practice the indivisibility of the educationist's responsibility, the insistence of the Central Advisory Council that "there is no objection on educational grounds to bringing into education anything of intrinsic interest by way of subject-matter or process that industry can provide" will be doubly welcome.

It would, however, be less than honest to suggest that the Council has, on the evidence afforded by "School to Life", fully appreciated the purpose of general education, as opposed to vocational training. Few, if any, educationists would quarrel with the conception of that "complete and generous education" which inspires and gives unity to the Education Act, 1944; but many would not be content, as are the members of the Council, to interpret their conception of a "complete and generous education" by reference to the opening words of the White Paper on "Educational Reconstruction": "The Government's purpose in putting forward the reforms described in this Paper is to secure for children a happier childhood and a better start in life; to ensure a fuller measure of education and opportunity for young people and to provide means for all of developing the various talents with which they are endowed and so enriching the inheritance of the country whose citizens they are". The fundamental weakness of this report—and it is one which largely invalidates the very real distinction which is drawn

between general education and vocational training—is the insistence of the Council that the final test of the educated man or woman is the "quality of the individual life". To emphasize unduly the "quality of the individual life" is to ignore the relation of man to society, the concept of democracy as no less a spiritual ideal than a system of government, and the responsibility of private judgment which true democracy imposes on every member of a democratic society and which it is the task of general education to inculcate. Vocational training, by equipping young people for certain tasks, is avowedly competitive in spirit and looks to worldly success. To know how to do something is to have, or seem to have, a possible start in life; and on any view vocational competence is a requisite of worldly advancement. But no society—and certainly no democratic society—can be organised simply in the interests of the fittest and hope to survive. If it were, it would cease to be a political society in the sense in which Aristotle understood the term, requiring a common equipment, open equally to the enjoyment of all, and created by a common effort of co-operation. Ideally, the success of an individual is meaningless or harmful, except as it is the mark of his superior service to the common good; and general education, if its purpose and spirit are rightly conceived, should represent a force in the curriculum of the school, and ultimately in society, which is not in the main competitive.

Of the need for such an interpretation of general education there is to-day no question. While differing abilities and aptitudes appear to be inborn and to grow independently of background and environment, they would seem to be drawn forth or repressed by the spirit of the age; but general education, if it is conceived as an antidote to the competitiveness of contemporary life, could be an essential means of preventing that narrowing of the potential gifts of the individual which competition enforces. More positively, the acceptance of a common core of training and knowledge, and its adaptation to meet the needs of differing abilities and aptitudes, could ensure that every young person, irrespective of his bents, learns to share "the common knowledge and the common values on which a free society depends". Even if there must be an academic bias in the education of some, a technical in that of others, and a modern (for lack of a better name) in that of the large majority, each could enter into that heritage of knowledge and values which is concerned to illumine the physical world, man's corporate life, and his inner visions and standards.

An ideal, but not impossible, vision of the democratic society of the future might see it composed of a myriad of smaller societies representing between them all the arts and insights of civilized man. It would be that they might participate in some of these, quite as much as for making a living, that education would prepare young people, and this participation would bring, in turn, that true "quality of the individual life" which is alone the test of the educated man or woman.

BACTERIAL STRUCTURE AND ACTIVITY

The Bacterial Cell

In its Relation to Problems of Virulence, Immunity and Chemotherapy. By Prof. Rene J. Dubos. (Harvard University Monographs in Medicine and Public Health.) Pp. xix + 460 + 22 plates. (Cambridge, Mass: Harvard University Press; London: Oxford University Press, 1946.) 28s. net.

IT has been thought that bacteria, with their comparatively simple morphology, are the most primitive forms of living things. It is only in recent years with the development of new techniques and the great advances that have been made, particularly in the field of bacterial physiology, and to a lesser extent in our knowledge of their structure, that their complex and diverse nature has been appreciated. This very diverse nature and the economic and medical importance connected with so many of the micro-organisms have resulted in bacteria being studied from many different angles. There has, in consequence, been a tendency for bacteriologists to remain limited to their own particular field. One of the outstanding values of this admirable book by Prof. Dubos is the co-ordination which he attempts between these various fields, so that one is enabled to view the bacterial cell and its multiple activities as a co-ordinated whole. It will go far to remove such obviously one-sided ideas as to regard the cell as a bag of enzymes or a mass of undifferentiated macromolecules. Dubos covers a wide field, and in the course of 460 pages cannot do more than give a bird's-eye view of the problems involved. Nevertheless, the bird is more of the nature of a lark than an eagle, and frequently comes down to earth to illustrate his points by specific examples. Indeed, there are more than a thousand references, although there is a tendency in the text to group these so that it is not always possible to trace the particular author of an observation cited.

Dubos starts by defining the problems he is up against. He stresses the impossibility of placing bacteria with any certainty into a phylogenetic scheme. His statement that "It is unlikely . . . that the distribution of metabolic characters can reveal the trends of physiological evolution and . . . any attempt to develop a phylogenetic system will have to be based . . . on what morphological characters are available" is open to question. The rather artificial phylogeny of Stanier and VanNiel which he appears to prefer is not very convincing.

Three chapters are devoted to the examination of the structure of the cell both directly, by staining and other microchemical and physical techniques, and indirectly by enzymes specific for various components of the cell, by immunology and bacteriophage action.

There is a natural transition from this to a chapter on bacterial variation. Here are discussed the normal variation occurring during the growth-cycle, adaptation and transmissible variation. In connexion with adaptive enzymes, the conclusion that their production "always involves synthesis of new protoplasm" although cellular division may be absent is no longer tenable in view of the recent work of Pollock and others, who showed that adaptation of both bacterial nitratase and tetrathionase may occur without increase in cell mass. In the case of transmissible variation involving environmental factors, Dubos

inclines to the view that this is a selection of an existing mutant. He quotes the work of Lewis on the selection of an existing mutant in *Bact. coli mutabile* very fully, but omits to mention the classical paper of Luria and Delbruck, who by the application of mathematical analysis to their experimental results showed conclusively that bacteriophage-resistant mutants are not induced but merely selected by the bacterial virus. The evidence for life-cycles and sexual reproduction is examined, and the verdict 'not proven' made. The chapter concludes with an account of one of the most significant advances in this field, namely, the work of Avery and his colleagues on the role of desoxyribonucleic acid in the pneumococcus on the production of its specific capsular polysaccharide and the transmutation of one pneumococcal type into another by this substance derived from the latter. It is enormously active: $1/6 \times 10^8$ being sufficient to produce the change, which is transmitted indefinitely.

In the following chapter, the question of virulence is discussed very fully. Dubos points out the vast number of unrelated factors which are concerned in this problem, any one of which might vary independently of the others. Quite apart from the possession of specific soluble antigens, toxins and the like, which no doubt are important factors, there appears to be more deep-seated cellular factors. A rough form derived from a virulent smooth organism regains its virulence when reconverted to the smooth form even when this is effected by smooth vaccines from avirulent organisms. Conversely, a rough form of an avirulent smooth organism remains avirulent even when a smooth vaccine from a virulent strain is used in the conversion to the smooth form. In the transmutation of pneumococcal types by the Avery technique the virulence of the new type depends on the virulence of the initial type from which the 'competent' rough form has been acquired.

Having dealt with the general structure and biology of the microbe, Dubos turns his attention to methods of destroying bacteria or preventing their multiplication. In the chapter on immunization against infection, he traces the historical development of the science of immunity and then considers the antigens of the cell which are responsible for elucidating the protective antibodies. These are not only the type specific antigens, but, as in the case of factors determining virulence, there is evidence that deep-seated non-specific antigenic substances are also responsible. These are present in the *R* forms. One example, however, that he quotes, namely, immunization with avirulent *R* forms of *B. anthracis*, is not really an instance of this, since immunity depends on an extracellular antigen actively produced by the living organism in the tissues of the host and not on a deep-seated antigen already present in the organism.

From this he passes on to consider the mechanisms of protective immunity and factors determining the preparation and standardization of adequate vaccines.

A long chapter is devoted to bacteriostatic and bactericidal agents and their mode of action.

He points out that there has been a return to the specific receptor theory of Ehrlich in the Woods–Fildes hypothesis of "specific poisoning of an essential reaction excited through an interference with a catalyst or essential metabolite". Many examples of this are given. Nevertheless, non-specific physiochemical reactions also occur. He gives examples of such reactions in the formation of unionized complexes between dyes and the acidic or basic groups in the organism, and in the action of surface-active substances. Many of these reactions are not entirely non-specific as they depend on particular acid or basic groups present in the cell. Differences in this respect are found between Gram-positive and -negative organisms and even between species very similar to each other, for example, *Bact. coli* and *Salm. typhi*. He discusses next the question of reversibility and the difference between bacteriostatic and bactericidal agents. This depends on a large number of environmental factors. It is often a quantitative rather than a qualitative difference.

In an addendum, Robinow describes nuclear structures in bacteria. It includes many excellent photographs of bacteria stained by his special method to demonstrate the nuclear apparatus.

This book will certainly rank with Landsteiner's "Specificity of Serological Reactions" as a classic in bacteriological literature. G. P. G.

PAINTER AND WRITER

A Canvas to Cover

By Edward Seago. Pp. 134 + 68 plates. (London: Wm. Collins, Sons and Co., Ltd., 1947.) 15*s*. net.

PAINTER and writer, yes, in that order. One feels that Mr. Seago will agree because he has called his very attractive book "A Canvas to Cover" and not "Paper to Write On". In this way he indicates that the picture comes first, and that his membership is of the band of painters who write rather than of the writers who paint. Thus he may escape the fate of Carl Larsson, who polished his prose with loving care and hoped it would be his memorial, but whose countrymen, deciding otherwise, obstinately continued to prize his paintings more highly than his writings.

Mr. Seago is a painter in the English tradition. He admires Constable—"the supreme master of landscape painting in terms of atmosphere and light"—Gainsborough, Crome and Cotman. Turner is not so sympathetic: "Turner was a great master—he was a caged being trying desperately to free himself". These are the main influences on the painter. But what of the writer? He has read widely; his pages contain quotations from Shakespeare to Shelley and Rupert Brooke, from Leonardo da Vinci to Ruskin and Roger Fry, but none of these has influenced his style, and Ruskin, surely, would have disapproved of "distant snow laying in a valley" (p. 104). At the same time, it is Ruskin who is quoted the most frequently, and that is no bad thing. Admiration of Constable coupled with a liking for Ruskin forms an excellent background for a painter.

The titles of his chapters and the wealth of illustrations show that the author's main concern is with the problems of painting out-of-doors, and it soon becomes evident that he is a close and careful observer of Nature in all her moods. Neither does he neglect the smaller things. He has noticed the behaviour of caterpillars on the railings of Berkeley Square, but is thoroughly confused about water-spiders (p. 72), unless, indeed, he uses 'water-spider' as a vernacular name instead of 'pond-skater'.

There is much sound sense in the text, a refreshing absence of 'isms' and theories, and a curious echo of Tolstoy in the phrase, "more simple people, living nearer the earth, are more touched by the essence of Art". Art itself is defined as "the expression of the senses through the imagination, and the basis is

truth", not beauty—"Beauty is certainly not Art". That, of course, is true enough if by beauty is meant merely the sum of those qualities which please the eye, but if beauty is given its philosophical content of wholeness, harmony and radiance, it immediately becomes the major part of art, and comprises the entire intellectual element.

But Mr. Seago is not concerned overmuch with philosophy, although certain of his difficulties can only be solved on that basis. His chief concern is with light, atmosphere and colour, and here he has much of interest and importance to impart. In his opinion grey is the most abundant colour in Nature, not green, and in Great Britain that is so. It is the dominant greyness of the atmosphere which harmonizes colour in Britain to a degree unequalled anywhere else, and which gives to our landscape a quality of serenity and repose lacking in more arid climates. The whole book is full of notes on colour; indeed, one of its strongest points is the way in which the painter's vision is translated into terms the layman can understand. If the artist cannot make us see with his eyes he has done his best to tell us what to look for with our own.

The plates are uneven, for the simple reason that not all the originals are suitable for reproduction in monochrome. As a general rule, swift sketches done with a heavily charged brush do not come out as well as pictures with more finish. Apart from all other considerations, the impasto of the sketch is often a disturbing factor; examples are the spotty, 'pimple-faced' appearance of "White Sails" (facing p. 92), and the fussiness of "Sunlit Spire" (facing p. 113). It would have been better to have reduced the number of photographs to a half or even a quarter, and to have given four more coloured plates in their place. To do so would have added still further to the value of this interesting and stimulating book.

A. TINDELL HOPWOOD

PHILOSOPHY IN ACTION

Power and Events

An Essay on Dynamics in Philosophy. By Andrew Paul Ushenko. Pp. xxiii+301. (Princeton, N.J.: Princeton University Press; London: Oxford University Press, 1946.) 30*s*. net.

IT is becoming clear that, among its other characteristics, the present intellectual barometer, especially in the United States, is set for 'change' rather than for 'fair'. Supercharged clouds are racing across the horizon, and it would not, one imagines, take a great deal to bring the whole thing down in a deluge. The mental image produced by this book is of that kind. A storm is a grand sight, and the pages before us contain grand thoughts. Of that there can be no doubt; and what they have to say is something like this. Classical philosophy was static in ideal and timeless in form. We call it 'taking long views'. The breakaway to a species of romanticism, mixed with more than a trace of the empirical, has produced a clarion call for progress, movement and intuition, with short shrift for analysis. Briefly, then, 'short views', or relatively so.

Dr. Ushenko attempts a reconciliation between these extremes by reviving Aristotle's *potentiality-actuality* as *power-event*. Naturally, there are significant differences, as when the author presents a respectable demonstration that power is an observable entity. Moreover, he adopts the wholly admirable principle of looking for a species of invariant, which might form the desired sheet anchor in transformations from one *Weltanschauung* to another. In this appears the possibility of a metaphilosophic system alongside contemporary work in metalogic and metamathematics. All well and good: what is surprising and encouraging is the setting selected for one of the major manifestations of power. This is none other than art, and the visual arts in particular. Indeed, it may be said that the transition from a typical Italian primitive painting, through the rhythm of the High Renaissance, to the romanticism of the Baroque, demonstrates very accurately corresponding events in the region of pure thought.

Several famous pictures are therefore discussed in terms of vector properties, namely, the capacity of implicit lines to carry over the eye to some essential point of the composition, and thus to generate æsthetic tension. That this is a very real effect was recognized years ago by Birkhoff in his "Aesthetic Measure". It is also amenable to qualitative analysis on a *Gestalt* basis, as I have shown myself in another connexion. A beautiful example given by Ushenko is Leonardo da Vinci's "Last Supper", though what can be learnt from it is perhaps not quite what the present author imagines. He quotes Wölfflin to the effect that the sensation of power is produced by the deliberate use of too short a table, and consequent crowding of the figures. Actually it is more likely that this feeling is caused by the step-wise grouping (not necessarily crowding) of the apostles about an axis passing through the Central Figure, and the 'reflexion' back to the middle suggested by the horizontality of the arms on the extreme right. But above all is Leonardo's choice of the moment of betrayal rather than the conventional one of Communion, as the theme of this sublime fresco. It connotes not peace, but a sword. All this reinforces the general argument in favour of *Sturm und Drang*, or at least a condition of dynamic equilibrium, such as Kekulé once invoked to establish the permanence of his benzene ring.

Thus it is that in the whole section dealing with power the author seems to be at his best. This is his own domain in which he is thoroughly at home. Outside, he discourses upon paradoxes, intuition, probability and so forth. The result here is less striking, and it might be prudent to regard these passages more as valuable comment than as new material. Apparently it is hoped that students will use the book as a text. If they do, they will need some kind of summary at the end of each main chapter (four in all), not as a temptation to skip the rest, but because a novice could not reasonably be expected to have a steady enough hand (and possibly head) to decant the liquor without undue spillage.

The general format is excellent, and the volume is a pleasure to handle. A minor blemish occurs in Fig. 1 (after Köhler). The segmental boundaries are drawn as dotted lines, whereas in the original (English edition) they are solid. The latter is much to be preferred for the fixation test.

To return to where we started. Dr. Ushenko's *Dynamik* is apt to run hot occasionally, and his reactions are a trifle gregarious. No harm whatever, though some of us may conceivably cherish something a little more *intime*, like a certain author of last century who honoured his thoughts by calling them the "companions of my solitude".

F. IAN G. RAWLINS

The Earth and the Stars

By Dr. C. G. Abbot. Pp. xii+288+29 plates. (New York: D. Van Nostrand Co., Inc., 1946.) 3.75 dollars.

THERE are many good books on astronomy intended for the general reader. This book cannot be included among them. It is a revision of a book, bearing the same title, which was published in 1925. But the revision has not been made with due care, and there are numerous errors and inaccuracies. The book is presumably intended primarily for American readers; but this does not justify credit for stellar parallax determinations being given only to American observatories, and the important contributions of the Greenwich and Cape Observatories ignored. The value for the solar parallax, derived from the observations of Eros at the opposition of 1931, is given wrongly. The parallax of Nova Persei 1901 is incorrectly stated; it is the value derived by a method which, as has been often pointed out, is invalid. The only determination of the constant of aberration which is mentioned is an old determination by Doolittle; there are many more recent and more accurate determinations. In view of the identification by Edlén of the origin of the coronal lines, the statement that the green coronal line is assigned to some ionized state of some common element could be improved upon. It is twice mentioned in the same chapter that Harlow Shapley has suggested a total of two hundred billion stars in our own galaxy. The sentence, "total eclipses of the Sun may occur in any month of the year", may be easily misinterpreted by the reader. In discussing the broadening of lines in stellar spectra no mention is made of rotational broadening. There is no continuity of plan in the book, which is scrappy and discursive. The publisher's note describes the book as "the absorbing story of the enigma of the universe: of the stars and constellations and their relationship to the earth". This seems to be a singularly inaccurate description of a book which is not worthy of its author.

La conquête de l'énergie atomique

1: Des Rayons uraniques à la Scission de l'Uranium (1896–1940); 2: De la Scission de l'Uranium à la Bombe atomique (1940–1945). Par Gerard de Vaucouleurs. (Actualités scientifiques et industrielles, 1000.) Pp. 114. (Paris: Hermann et Cie., 1946.) 120 francs.

OF the millions of words that have been written about nuclear energy, almost all are in highly technical (and mostly still secret) reports, or in articles and booklets that seek to explain the mysteries of nuclear physics and 'atom' bombs to the world and his wife.

M. de Vaucouleurs has chosen a middle path; he claims, in his preface, the distinction of not being a specialist in the field of which he writes, and he explains that his little book grew out of an article originally published by the French and Belgian astronomical societies. He has written, in scientific but non-specialist language, a sober but interesting account of the growth of nuclear physics from 1896 until 1940, followed by what is essentially a summary of the official American and British statements on the later developments that culminated in the explosions at Alamogordo, Hiroshima and Nagasaki. It can be read with ease and interest by anyone of scientific education, nuclear physicists included; no expert knowledge of French is needed, for M. de Vaucouleurs writes very clearly.

Names and dates are numerous, and with the aid of *Physics Abstracts* it is easy to locate the many published papers that the author has consulted. The reviewer applied his usual test of typographical accuracy by looking for the third 'c' in Prof. Cockcroft's name, and found it was not always there.

P. B. MOON

Faraday's Encyclopedia of Hydrocarbon Compounds

(Covering the Literature up to January 1st, 1946). Compiled by Dr. Joseph Escott Faraday. Vol. 3: C_3. Pp. cxii+457. (Manchester: Chemindex, Ltd., 1947.) £5 10*s*. 0*d*.

THE appearance of this volume means that the literature is now covered completely to January 1, 1946, for all hydrocarbons C_1–C_3, and by the end of next year it is hoped to publish addenda which will bring the whole work up to the end of 1947; the author has always kept his promises in the past and there is no reason for supposing he will not deliver the goods in the present instance. This will be no mean achievement.

The plan of the present volume must now be familiar to all organic chemists, as it is the same as that of the last; this is a proof, if one were needed, of the excellence of the original arrangement. The value of a compilation of this kind cannot be overrated. The necessity of searching Beilstein and the indexes of numerous volumes of *Chemical Abstracts*, etc., is obviated and all information, completely up to date, is contained in one place. The tedious and time-consuming literature search is a thing of the past, and would that a book such as this were available for all organic compounds and not only for hydrocarbons. Perhaps collaboration between the author and Dr. Dyson is indicated.

One improvement has been introduced: in the case of compounds where a large number of methods of formation are found—in the case of ethylbenzene, for example, there are 212—those that are likely to prove of use in the laboratory are clearly indicated.

The value of a book of this sort depends, of course, entirely on its accuracy, and although the reviewer does not claim a complete check, no errors were detected. The price of £5 10*s*. appears very reasonable for these times, and all chemical libraries should possess a copy.

Nature's Undiscovered Kingdom

By Walter J. C. Murray. Pp. x + 98 + 15 plates. (London: George Allen and Unwin, Ltd., 1947.) 7*s*. 6*d*. net.

THIS is a charming little book on the life of the countryside, from the wide landscape swept by wind and weather down to spiders in the herbage and moths fluttering through the twilight. A tendency to a somewhat "lyrical style" of writing (the words are borrowed from the publishers' note) is compensated for by accuracy of observation and description. The account, for example, of watching and photographing young grey squirrels is quite delightful. Mention of these squirrels reminds us that the illustrations are all from the author's own camera and deserve the fullest praise, in particular the portrait of a grey squirrel facing p. 23. Why the book was given the title of "Nature's Undiscovered Kingdom" eludes me, for this is certainly the kingdom into which Jefferies and Hudson pioneered many years ago: the author is a worthy follower. F. PITT

THE TRANS-URANIUM ELEMENTS

By Prof. GLENN T. SEABORG and Prof. EMILIO SEGRÈ
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BEFORE 1940, chemistry did not know of any element with atomic number greater than 92 (uranium). In spite of occasional claims to the contrary scattered throughout the literature, there still is no good evidence that such elements exist in Nature, except perhaps for minute amounts formed from uranium by neutrons of various origin. This finds its explanation in their instability, since their radioactive half-lives are now known to be small compared to geological times. The last seven years have seen the first positive identification of four trans-uranium elements, of atomic numbers 93, 94, 95 and 96, all discovered as a result of their synthesis by transmutation reactions starting with uranium as the primary material.

The first serious possibility for producing trans-uranium elements was opened by the discovery of artificial transmutation and artificial radioactivity, and particularly by the process of neutron capture found by Fermi and his colleagues in 1934[1]. The new isotopes formed by the capture of neutrons most frequently decay by negative beta-emission, to form elements with atomic numbers increased by one unit, and these workers asked themselves whether the bombardment of uranium should not lead to the element with atomic number 93. At the time they did not suspect the existence of the fission process, and hence they tried to interpret their observed complex of fission-product activities in terms of elements near uranium. The chemistry of the trans-uranium elements was, of course, not known ; it was rather generally thought that they would be like rhenium, osmium, etc., the elements immediately above them in the same vertical column of the Periodic Table. Therefore, when their chemical tests showed that the induced activities were not due to elements of atomic numbers 86–92, these investigators naturally interpreted them in terms of several series of trans-uranium elements.

At this point the study of these substances was taken up by Hahn, Meitner and Strassmann[2], and for several years they interpreted their work on the trans-uranium assumption ; but gradually difficulties piled up to the extent that this interpretation no longer seemed possible. Further study by these investigators[3] and by I. Curie and Savitch[4] showed that the trans-uranium hypothesis could not be considered satisfactory, since the number of distinct radioactive bodies became too large to fit simply into such a picture. The riddle of the many activities was finally solved by Hahn and Strassmann[5] in 1939 with the discovery of the fission process. Following this great discovery, many workers succeeded in identifying practically all the recognized activities as fission products. However, the important activity of 23-min. half-life, which had previously been assigned by Hahn, Meitner and Strassmann[6] to U^{239}, was confirmed to have been correctly assigned.

As the news of the discovery of fission spread rapidly over the world, many experimental investigations of this process were made. Of special importance to the subject of the trans-uranium elements were some measurements of the range of the fission fragments by the simple method of finding the distribution of fission-product activity in a set of thin foils placed in contact with neutron-irradiated uranium. This was done by many investigators, including Joliot[7] in Paris, Anderson and co-workers[8] in New York and McMillan[9] in Berkeley, California. A by-product of the latter investigation proved in the end to be more important than its original purpose, for in the thin layer of uranium oxide used was found a non-recoiling activity with a 2·3-day half-life, in addition to the expected 23-min. activity ascribed to U^{239}.

This 2·3-day body was immediately suspected to be the daughter of the 23-min. uranium, and hence was suspected to be the isotope 93^{239}. Experiments were made by Segrè[10] to test this genetic relation, but because of the ease with which element 93 exhibits some of the chemical properties of the rare earths, they were interpreted as meaning that the 2·3-day body was a rare-earth isotope.

Final clarity on the subject was brought about by the later work of McMillan and Abelson[11] in Berkeley, which led to their positive identification of a radioactive isotope of element 93, the first trans-uranium element to be discovered. They worked with pure samples of the 2·3-day activity, prepared by exposing thin layers of uranyl nitrate to neutrons from the 60-in. cyclotron and thus allowing the fission fragments to escape by recoil. By a series of chemical experiments they were able to show that the 2·3-day body has at least two oxidation states, the lower one showing chemical properties like the rare earths (and uranium IV and III) and the upper one behaving like uranium VI. They also demonstrated the growth of this body from the 23-min. U^{239}, thus establishing its identity as 93^{239}. Their results were later confirmed by Starke[12] and by Strassmann and Hahn[13] in Germany.

At this point it became obvious why element 93 had evaded earlier seekers, since its chemistry was more like that of uranium than that of rhenium, the most obvious difference from the former being the greater relative stability of the lower oxidation state or states. This similarity to uranium led McMillan and Abelson to suggest that a new series of elements of closely related properties was starting here, similar to the phenomenon which occurs earlier in the Periodic Table with the rare earths. Because of this relation, and the fact that uranium was named after the planet Uranus, McMillan proposed the name 'neptunium' for element 93 and 'plutonium' for its daughter element 94, which was, however, as yet unidentified.

The next step in the investigations at Berkeley was the identification of the element with atomic number 94. It was naturally supposed that 94^{239} would emit alpha-particles, but a search for them in decayed 93^{239} samples showed an activity too weak for good experimentation. Hence McMillan tried bombarding uranium with deuterons, hoping to produce another isotope of element 94 with a shorter half-life. This

hope proved to be justified, and he found a definite alpha-activity, giving particles of a range near that of polonium. He was able to say in the summer of 1940 that the body responsible was not protactinium (element 91), uranium or neptunium, but had chemical properties resembling those of thorium (element 90). (This was shown by precipitation as the iodate using Ce (IV) as carrier.)

At this time a change in the nature of the investigation was taking place. The recognition by many people of the military possibilities of fission was having its effect, and the change to an organised programme under military secrecy was inevitable. McMillan was called away for research in the field of radar, and the work was continued at Berkeley by a group including Kennedy, Seaborg, Segrè and Wahl, at first as their own unsponsored research programme but later as part of the organised national war effort. During late 1940 and early 1941, the key to the chemistry of element 94 was found[14], using the isotope resulting from the deuteron bombardment of uranium, which was later shown to be largely 94^{238}. With a very strong oxidizing agent it was found possible to oxidize this element from the IV or III oxidation state to a higher state which is not precipitated as a fluoride, and thus to show that it was indeed a new element. These investigators adopted the name 'plutonium' for element 94, following McMillan's lead for naming trans-uranium elements after the planets beyond Uranus, and they suggested the chemical symbols 'Pu' for plutonium and 'Np' for neptunium. At about the same time, Pu^{239} was finally identified, and the military possibilities[15] of this isotope were established by the experiments of Kennedy, Seaborg, Segrè and Wahl[16], who measured the fission cross-section of Pu^{239} using a sample prepared by decay of Np^{239} and later further purified by taking advantage of the then known chemistry of plutonium. It is, of course, impossible within the scope of this article to describe the development of the chemistry, metallurgy and technology of plutonium from a tracer scale, through the ultramicrochemical or microgram scale and on up to the kilogram scale, and this phase of research and development has been dealt with partially elsewhere[17].

The investigation of the early chemistry of neptunium was limited to studies by the tracer technique using the short-lived isotope Np^{239}, as described above. Fortunately, there is another isotope of neptunium, Np^{237}, which is sufficiently long-lived to make it possible to work with weighable amounts using the methods of ordinary chemistry. This isotope is the decay product of the previously known 7-day beta-particle emitter U^{237}, which is formed[18,19] as the result of an $(n, 2n)$ reaction on U^{238}. This neptunium isotope, first observed, in Berkeley, by Wahl and Seaborg, is an alpha-emitter of very long half-life, $2{\cdot}25 \times 10^6$ years. Very fortunately, it is produced in the large uranium chain-reacting units in weighable amounts, so that the chemical properties of neptunium can be, and to a large extent have been, studied by the methods of ordinary chemistry and microchemistry.

More recently the next two trans-uranium elements, with atomic numbers 95 and 96, were discovered. During 1944 and 1945, Seaborg, R. A. James and A. Ghiorso, working at the Metallurgical Laboratory of the University of Chicago of the Manhattan District, identified in the products of the bombardment of Pu^{239} with 40 MeV. helium ions in the Berkeley cyclotron, two isotopes of element 96 of mass 240 and 242, that is, 96^{240} and 96^{242}, formed by (α, n) and $(\alpha, 3n)$ reactions. These isotopes are alpha-emitters with half-lives of one month and five months, respectively. At nearly the same time but somewhat later, Seaborg, James and L. O. Morgan at the same Laboratory identified an isotope of element 95 with mass 241. The 95^{241} is the daughter of a relatively long-lived beta-emitting Pu^{241}, and emits alpha-particles with a half-life of about 500 years. This work was made possible by the vital participation and co-operation of J. G. Hamilton and his group at the University of California, who had recently rebuilt the Berkeley 60-inch cyclotron to produce the required high-energy particles, and who performed the bombardments.

The chemical investigations on the tracer scale have shown that these 'trans-plutonium' elements are almost exclusively tripositive in aqueous solution. They behave chemically very similarly to the tripositive rare earths and can be separated, especially element 96, from the rare-earth elements only with difficulty, using fractionation methods. Apparently, stable tripositive element 96 contains seven *f* electrons with a structure analogous to stable tripositive gadolinium.

It is not possible, of course, to name elements 95 and 96 after planets beyond Pluto, since no such planets are known. These investigators have suggested the name 'americium', symbol 'Am', for element 95, and the name 'curium', symbol 'Cm', for element 96. These are suggested on the basis of the similarity with respect to *f* electrons of the electron structure of element 95 and the rare earth europium, and of element 96 and the rare earth gadolinium. Thus element 95 would be named after the Americas, or New World, by analogy with europium, which was named after Europe, and element 96 would be named after Pierre and Marie Curie, historical leading investigators in the field of radioactivity, by analogy with gadolinium which recalls Gadolin, the investigator of the rare earths.

There has been a great deal of speculation throughout the years as to the possibility of the existence in Nature of isotopes of the trans-uranium elements. The present state of knowledge of their chemical properties makes it possible to make efficient and meaningful searches for such elements. In fact, Seaborg and M. L. Perlman, early in 1942, made a chemical separation of neptunium and plutonium from a quantity of pitchblende ore, and were able to show the presence of a small quantity of alpha-activity in this trans-uranium fraction which they attributed to plutonium, the isotope Pu^{239}. The amount of plutonium in the pitchblende corresponded to about 1 part in 10^{14}, an amount which could not possibly have been found had the chemical properties not been known. This plutonium isotope is present in such uranium-containing ores, in spite of its relatively short half-life, probably because it is continuously formed by the absorption in U^{238} of some of the neutrons which are always present. A particular source of neutrons, sufficient in amount to account for the presence of this amount of plutonium, results from the spontaneous fission of uranium. It seems likely, in view of their relatively short half-lives, that the other trans-uranium elements could be present only as a result of a similar mechanism for formation, and hence will be present in such ores only at concentrations even smaller than that found for plutonium.

In conclusion, we wish to thank our colleague Prof. E. M. McMillan for supplying part of the material for this article and for helping in its preparation.

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THE ZOOLOGICAL SURVEY OF INDIA

By Prof. H. MUNRO FOX, F.R.S.
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THE Zoological Survey of India was founded in 1916. It derived from the Zoological Section of the Indian Museum. The latter was a descendant of the zoological portion of the Asiatic Society of Bengal. Annandale, a first-class naturalist and an authority on freshwater faunas, was the first director of the Survey. He had distinguished successors in Kemp, Sewell, and Prashad, and, when the last-named was lately called to be adviser on fisheries to the Government of India, in Dr. B. N. Chopra, the present acting director. Other members of the Survey who have achieved a high reputation are Dr. S. L. Hora, who for the past few years has been director of fisheries in the Province of Bengal, and Dr. H. S. Pruthi, recently selected to be director of the new Plant Pest Protection Bureau. From the beginning, however, the staff was small, and unhappily retrenchment in the early nineteen-thirties so much reduced it that the Survey was forced almost to abandon field work and to restrict itself to museum activities, thus cutting out its main purpose, namely, the animal survey of India. Indeed, the staff then became so small that even the proper care of the large and valuable collections became difficult.

The objects for which the Survey was founded were the following. First, it was to undertake faunistic studies so as to acquire all possible information on the geographical distribution of Indian animals; secondly, to maintain and add to the national zoological collections started nearly a century and a half ago by the Asiatic Society of Bengal; thirdly, to maintain and develop the public exhibits in the Indian Museum; fourthly, to identify specimens as required by investigators in any part of the country; and fifthly, to advise the Government on zoological matters. It is clear that the Survey has the functions of the British Museum (Natural History) in London with the obligations of the field survey in addition. This is an immense task in a country of the size of India.

In zoology, as in all the sciences, investigators will go on making discoveries for their own sake, from purely scientific curiosity. This is how fundamental science advances. Some of these discoveries are from time to time made use of for human benefit; no one can foretell which of them will be useful. The Zoological Survey of India functions in just this twofold way, and must continue to do so. Discoveries made by its staff in the field and in the laboratory may any of them be of inestimable value to India. This applies to medical zoology, which means animal pests and animals which transmit pests, to veterinary zoology, to agricultural science and forestry We may take one case a little more in detail, a case which is particularly important in view of the food shortage of to-day, namely, the fisheries. There is great need of developing and expanding the Indian fisheries, both marine and freshwater. More fish should be got out of the Indian seas, always taking care to avoid the danger of overfishing. Inland fishes could be more extensively and better grown in tanks. For all this, much knowledge is needed of the lives of the fishes. No doubt many of the necessary studies will be made in the universities, but the Survey must be the centralizing body, with its collections for the final precise identifications. It is seldom realized by non-biologists that all investigations must be based on the bedrock of accurate determination of the species of organism studied, and this is very much a specialist's responsibility. For development of the Indian fisheries there is also an urgent need for the establishment of at least one marine biological station, perhaps at Karachi where the fauna is good, and this should be closely linked to the Survey. It must be a research station such as that of Plymouth, with a permanent staff, adequate modern equipment, a research vessel, and a library.

In 1880 the Secretary of State for India commenced publication of the important series of monographs that bears the title of "The Fauna of British India". The early authors of these monographs were British, resident in India; for example, Dr. N. Annandale, Lieut.-Col. H. H. Godwin-Austen, Lieut.-Col. C. T. Bingham, W. T. Blandford, Lieut.-Col. Sir S. R. Christophers, Francis Day, Lieut.-Col. F. C. Frazer, G. F. Hampson, and Lieut.-Col. J. Stephenson. The first Indians to prepare monographs were S. Maulik, then professor of zoology in the University of Calcutta, and Dr. B. L. Bhatia; future volumes have been entrusted to Dr. Bhalerao, Dr. Chopra, Dr. Hora, Dr. Prashad and Dr. Sharif. With the introduction of self-government, the responsibility for the continuation of this series falls on the Government of India, and the editor of future volumes will be the director of the Zoological Survey of India: it is expected that many of the authors will be Indians, and the source to which one looks primarily for such experts is the Zoological Survey.

Last year Lieut.-Col. R. B. Seymour Sewell, formerly director of the Survey, returned to India at the invitation of the Government to submit proposals for the reconstruction and expansion of the Survey. His report was issued in November.

It is a document of great importance to the future of the Survey and of India. Sewell makes it clear that an enlarged Survey is essential to the scientific and economic advancement of the country. There should be a very considerable expansion of the staff. Originally there were four officers, then seven, now only five. There should be at least two dozen in the near future, so that each branch of the animal kingdom shall have its expert. One of the new officers would be naturalist to the Marine Survey. This would be a revival of the surgeon-naturalist's post, so fruitful in the past in the deep-sea work of R.I.M.S. *Investigator*.

The immediate difficulty will be to fill these zoological posts. Early this year I was in India as a delegate to the Indian Science Congress and then visited a number of the universities. Some of the zoology departments were excellent, others less good; but in most of them the standard could well be raised. In Indian universities zoology has not been so much developed as some of the other sciences. This is, no doubt, a matter of tradition, hard to change. In addition there are in India very few field naturalists. This means that first-class personnel for expanding the Survey will not easily be found. Sewell has put forward a scheme for the training of young Indian zoological graduates of the universities for two years in the Survey and then for a year abroad.

The Survey, with all its valuable collections and library, was moved for safety during the War from Calcutta to temporary quarters at Benares. There it is to-day, quite inadequately housed. There is insufficient room either to tend or to consult the collections, and the building is liable to floods which have already damaged valuable books. The Survey has the best zoological library in India, probably in Asia. Sewell recommends the erection at New Delhi of a new home for the Survey in a fire-proof, air-conditioned building. Here it would be placed centrally for Government consultation, situated near other scientific institutes.

When India assumes independence she cannot afford to neglect the expansion of her Zoological Survey.

NEW PHYSICO-CHEMICAL PHENOMENA IN THE DEFORMATION AND MECHANICAL TREATMENT OF SOLIDS*

By P. REHBINDER

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THE mechanical structural properties of solids—elasticity, plasticity and strength—are of paramount importance in most branches of modern engineering. These properties, which are determined by molecular interaction, are usually investigated without resorting to advanced physical chemistry. However, the methods of physico-chemical investigation of the deformation of solids, with due recourse to the factor of the chemical composition of the surrounding medium, lead to new methods of controlling the mechanical properties of solids and to novel methods of their mechanical treatment.

* Summary of a report at a general meeting of the Academy of Sciences of the U.S.S.R. in July 1946.

The earlier investigations by the author and his associates have established the fact that the addition to the surrounding medium of small amounts of the so-called surface-active substances, the particles of which are adsorbed on solid surfaces, is capable of considerably lowering the resistance of a solid to stress.

These very general phenomena of facilitating strain and disintegration of solids by adsorption have been extensively utilized by the author and his co-workers, and afterwards by many other investigators, both in the Soviet Union and abroad in various practical fields. For example, they have been used to facilitate the boring of stable (hard) rocks, in various processes of disintegration of solids by milling, in cutting and pressure working of metals, etc. It is this phenomenon of adsorptional promotion of strain that largely accounts for the effect of cooling lubricants, in the absence of which some of the above processes become altogether impracticable. The theory of this phenomenon must underlie any scientifically based selection of these lubricants.

During the Second World War, mainly in connexion with the application of these investigations to the machining of metals, we succeeded in elucidating to some extent the mechanism of the adsorptional promotion of strain and, at the same time, in discovering a number of new phenomena in this domain.

It was ascertained that these phenomena are associated with the penetration of surface-active substances into the depth of the strained solid along the surface of the wedge-shaped ultra-microscopic cracks which are developed in course of deformation. After unloading, that is, removal of the stress, the micro-cracks become closed again, the surface-active substances adsorbed on their inner surface are expelled by molecular cohesion and the original mechanical properties of the solid are restored.

The closing of the micro-cracks after unloading is a process involved not only in elastic but also in plastic deformations, with the sole difference that, after shear involving slippage, the closing of the cracks takes place at new points of their surfaces.

These investigations have led to the discovery of some new phenomena which have been described in the report referred to above, and partly demonstrated in a micro-cinematographic film. Special interest is presented by two effects resulting from the penetration of surface-active substances into the micro-cracks: the retarded elastic deformation in mica crystals and the structural changes on plastic deformation of metals.

When a sheet of mica is bent or stretched in absolutely dry air, the deformation occurs practically instantaneously as soon as the external force is applied, and disappears also instantaneously after removal of the stress when the latter is below the elastic limit. In the presence of moisture, however—in water vapour or when the crystal is wetted with water—a gradual increase of deformation on application of stress is observed and a gradual decrease on removal of the stress. On addition to the water of organic substances readily adsorbed on mica, for example, alcohols, this retardation is still further enhanced, and the time required for the deformation to attain its maximum or to disappear after unloading is a matter of some three days. Complete recovery is observed in the case of these phenomena.

So long ago as in the experiments of I. Obreimov on the cleaving of mica along the cleavage planes *in*

vacuo, it was demonstrated that the cleft formed by introduction of a glass wedge is closed again on removal of the latter, so that subsequent cleavage requires almost the same amount of mechanical work. This phenomenon underlies the ingenious method suggested by I. Obreimov for measuring the surface tension at the cleavage faces of mica. The cleavage of mica in such experiments is quite similar to the formation of wedge-shaped micro-cracks, which, according to our concepts, are developed in the region of elastic deformations without any rupture of the solid. As has been kindly pointed out to us by Prof. I. Frenkel, such a concept of reversibly closing micro-cracks leads to the elimination of some grave flaws in Griffith's well-known theory and opens new ways towards its substantial improvement.

In studying the plastic flow in single crystals of the purest tin, it was established that under the action of the surface-active substances (for example, oleic acid or cetyl alcohol) added to the surrounding inert medium (purified paraffin oil), the number of the layers formed which slip over each other on extension of the single crystal increases fifteen-fold or more, whereas their thickness decreases in proportion. These phenomena were observed by means of accelerated micro-cinemaphotography of the stretched single crystals. The sorption of the organic surface-active substances by the strained metal lowers the yield value of tin to half the original value, facilitating the flow by a kind of 'internal' lubrication of the slip planes. Such sorption manifests itself in the abrupt fall of the electrical conductivity of tin, which on removal of the external stress is gradually restored as the original mechanical properties of the metal are restored.

In the case of ordinary, small-grained zinc and copper, the strain-hardening of the outer layers was found to increase sharply after periodic deformation in the presence of surface-active substances. The adsorptional lowering of the hardness of the metal originally observed on deformation is followed by its sharp increase due to a secondary phenomenon: strong disintegration of the crystal grains or their orientation (formation of texture), which always leads to hardening. Some new electrocapillary phenomena were also observed and investigated, namely, the lowering of the hardness or strength of metals in solutions of electrolytes when the surfaces of the micro-cracks formed are charged electrically, irrespective of the sign of charge.

The seat of the above peculiar surface phenomena is thus the inner surface of the micro-cracks which are developed in a strained solid. The latter is, as it were, a kind of labile adsorbent, the newly formed surface of which gradually disappears again after unloading. The whole set of phenomena of adsorptional promotion of deformation in a solid can be regarded as evidence in favour of our concepts regarding the continuous development of micro-cracks in the process of deformation, beginning from the region of elastic deformations.

The elucidation of these phenomena is evidence of the fruitful result of resorting to physical chemistry in the problems of physics and mechanics of solids. This boundary domain of science can in general be said to be of great practical importance in the solution of the problems involved in the working and machining of metals and other solids, as well as in the theory of wear, creep, corrosion, fatigue and a number of other problems in the applied physics of metals and building materials.

THE FIRST CHEMICAL JOURNAL

By Prof. JAMES KENDALL, F.R.S.

IN connexion with the forthcoming centenary celebrations (postponed from 1941) of the Chemical Society, the discovery of the first volume of the proceedings of an older organisation in the same field is of timely interest.

This volume—a well-bound folio of 452 pages in copper-plate manuscript, inscribed simply "Chemical Society instituted in the beginning of the year 1785" —was presented to the Royal Irish Academy by Sir William Betham on January 26, 1846, and lay unnoticed in its library for more than a century. Recently, however, it attracted the attention of the Rev. P. J. McLaughlin, of St. Patrick's College, Maynooth, and through an article written by me in the July 1942 number of *Endeavour*, Dr. Farrington, librarian of the Academy, and Dr. McLaughlin were able to identify it as Volume 1 of the *Proceedings of the Chemical Society of the University of Edinburgh*. Practically all the thirty-two communications which it records *in extenso* represent, in fact, papers read at meetings of this Society by members whose names are on Joseph Black's "List of the Members of the Chemical Society, 1785".

That it should have found a resting-place in Dublin is not really surprising. The nationality of nineteen of the fifty-nine original members of the Edinburgh society has been definitely established; of these only three were native Scots, three English, and the residual thirteen were Irish! Presumably the unknown secretary, to whose admirable diligence the volume owes its existence, was one of this majority and retained possession of his handiwork on his return to Ireland.

At my request, the Council of the Royal Irish Academy agreed to send the volume on loan to the Royal Society of Edinburgh for my inspection. Unfortunately, the communications do not include anything of major scientific importance, and my expectation that the volume might afford a unique record of contemporary chemical thought has not been realized. I had hoped to find some lively discussions (with Joseph Black himself participating) on the revolution that Lavoisier was then leading against the phlogiston theory, but this is rarely even mentioned. Most of the papers are routine descriptive chemistry of the classical period, and the most promising of the few exceptions, "an attempt [by Mr. Thomas Beddoes, later director of the Pneumatic Institution and supervisor of Humphry Davy] to point out some of the consequences which flow from Mr. Cavendishes discovery of the component parts of water" peters out, after an attractive start, into an addendum entitled "a conjecture concerning the use of Man"re"!

This disappointment, however, emboldened me to make a second request to the Council of the Royal Irish Academy, that it should present the volume to its original owners, and to that request it has now graciously consented. The oldest chemical society in the world will certainly treasure the first volume of its proceedings as a priceless possession. Even if it has no great intrinsic value, it does after all constitute the first journal extant of a purely chemical character, the *Annales de Chimie* being initiated just five years later; and extracts from it may well prove worthy of wider publication.

OBITUARIES

Prof. Edouard Chatton

EDOUARD CHATTON, professor at the Sorbonne and director of the Marine Biological Laboratory at Banyuls-s-mer, corresponding member of the Paris Academy of Sciences, died on April 23, at the age of sixty-three, after a long illness. The French protozoological school loses one of the best—and probably the most brilliant—of its research workers.

Chatton was a pupil of Yves Delage. In 1907 he became Felix Mesnil's assistant at the Pasteur Institute in the department founded by Alphonse Laveran. Seriously wounded on the Champagne front in 1915, he went to Tunis, where he received the hospitality of Charles Nicolle at the Pasteur Institute. When Strasbourg returned to France, he was appointed as lecturer at the new University (1919), and on Bataillon's departure he became director of the Biological Institute. But his constant aim was to work in a marine laboratory. In 1932 he was elected professor at Montpellier and director of the Sète Marine Station. Finally, in 1938, he migrated to Banyuls-s-mer, one of the Sorbonne's marine stations, where he ended his life and career.

Although Chatton made fruitful incursions into the field of parasitic copepods of ascidians and sponges, and on lower parasitic Metazoa (*Orchitosoma*), almost all his activity was devoted to the study of Protozoa. His work increased to a notable extent our knowledge of numerous groups and has been the source of many general and theoretical implications.

Chatton started his scientific career in 1906 with the discovery of the parasitic dinoflagellates. A remarkably large number of new types was found, exhibiting all degrees and sorts of parasitism: external, intestinal, cœlomic, intracellular. The different types show profound changes in morphology, and in kinetics of cell cleavage; but the morphology of the spore remains that of a typical dinoflagellate. That was the origin of fruitful considerations on parasitism and its action on cellular morphology and physiology. From the study of the nucleus, he believed that *Haplozoon*, the curious parasite of annelids, was a parasitic dinoflagellate, a prediction which turned out to be true.

The careful observation of the coccidian nucleus led Chatton to conclude that Coccidia also had their origin among dinoflagellates; and after twenty-five years, shortly before the end of his career, he himself had the satisfaction of discovering the extraordinary dinoflagellates, the Coccidinides, which are parasitic in dinoflagellates. The life-cycle of these organisms is a coccidian one, but their gametes are typical dinoflagellates.

The study of nuclear fission in *Vahlkampfia* was the basis of our understanding of the type of mitosis in Protozoa and of the shattering of the "Nukleol-Centrosom" theory. When studying the life-cycle of *Entamœba dysenteriæ*, Chatton discovered the four-nucleate metacystic stage. He discovered also many new types of parasite and worked out the life-cycle of hitherto completely enigmatic organisms, such as *Pansporella*, *Siedleckia*, *Amœbidium*, *Sporomonas*, etc. He also devoted much of his time to the study of free-living and parasitic ciliates. The understanding of the affinities of the ciliates of the gills of Molluscs led to the exclusion of Hypocomidæ from the suctorians. The genesis of the sucker from a centrosome was one of the newest conclusions of this work.

The Apostomea led to much more important discoveries: the existence of two-host cycles in ciliates; the discovery and description of ciliary metamorphosis; the recognition of some of the general laws of the morphology of ciliates: the cinetosome (basal granule) is a self-reproducing body always located at the l ` of the ciliary 'line'; the trichocysts are formed by cinetosomes, as are the cnidocysts of the dinoflagellate *Polykrikos* (another of Chatton's discoveries). These general conceptions were verified on numerous other types of ciliates.

Chatton discovered also new Mycetes, the polysporal bacteria *Metabacterium*. He was the first to obtain the bacteriologically pure culture of an intestinal parasite—*Trichomastix* of the lizards.

Finally, to crown a fruitful career, comes the work accomplished with Madame Chatton on the determinism of sexual reproduction in some ciliates. With bacteriologically controlled cultures, the Chattons have shown that many conditions govern the determinism of copulation: a certain degree of inanition, the nature of the bacterial species present, a 'zygogenic factor' (calcium or ferric chloride, pyruvic acid, etc.) and a certain limiting *p*H value. The copulation of *Glaucoma scintillans* could even be produced at will in bacteria-free cultures.

This is only a brief summary of some of Chatton's investigations. Some three hundred papers represent the result of forty years of almost uninterrupted and concentrated work. Chatton had an extensive knowledge of zoology and parasitology, botany, cytology and microbiology. He possessed also, allied to an almost violent and exclusive love for Protozoa, a peculiar sense which enabled him to recognize every known protozoon, and to guess at first sight the life-history of so many mysterious micro-organisms. So has Chatton's work dominated protozoology. General cytology and protozoology itself have still much to gain from his theoretical conceptions.

Edouard Chatton's death deprives protozoology and biology of one of its most accomplished, brilliant and productive workers. ANDRÉ LWOFF

Mr. B. H. Barrett

BENJAMIN HILTON BARRETT, who died on June 9 after a short illness, at the age of fifty years, had for many years been a lecturer in geology at the University of Glasgow, where he graduated as a student of the late Prof. J. W. Gregory. His main interests were in stratigraphy, but he had made extensive contributions to the petrology of the red rocks of the west of Scotland, particularly of Arran and Dumfriesshire. He collaborated with Gregory in writing a book on general stratigraphy. His most important work, however, was probably that on the small coalfield of Canonbie, on the borders of Dumfriesshire and Cumberland. In collaboration with Dr. J. E. Richey he published, as a 'war-time pamphlet' of the Geological Survey, an account of this coalfield which greatly modified the interpretation of its sequence and structure, and indicated the possibilities of considerable developments in its concealed areas. For some time he was secretary of Section C of the British Association. He had also been an active supporter of the Glasgow Geological Society, of which he was president.

Barrett was a loyal colleague and a cheerful companion, a geologist of sane judgment whose early death is a loss to Scottish geology. A. E. TRUEMAN

NEWS and VIEWS

Scientific Advisory Department for Defence, Australia:

Mr. A. P. Rowe, C.B.E.

It has recently been announced that a Scientific Advisory Department for Defence is to be formed in Australia under Mr. A. P. Rowe. Mr. Rowe is probably best known as the war-time chief superintendent of the Telecommunications Research Establishment, Great Malvern, where most of the research on radar for the Royal Air Force was carried out. It was not, however, in the field of radio that his first research work was done, but in investigations into the ballistic properties of bombs. This work was carried out at the Air Ministry research station at Orford Ness, and notable contributions were made to the technique of high-level bombing. A. P. Rowe's connexion with radar began when he was appointed secretary to a committee under Sir Henry Tizard undertaking a scientific survey of the problems of air defence. He soon appreciated the importance of this new technique being developed by Sir Robert Watson-Watt and his colleagues, and it was in no small measure due to his vision and enthusiasm that research was intensified with the view of establishing quickly an early warning radar system and of investigating the application of radar technique to the problems of air warfare. In 1937 he succeeded Watson-Watt as superintendent of the Bawdsey Research Station. After the outbreak of war he gathered together many leading men of science, and the Bawdsey team grew into the large establishment known as T.R.E. (Telecommunications Research Establishment). He was quick to appreciate the extreme importance of centimetric waves for radar, and under his direction research in this field was pursued at high pressure and led to many of the outstanding radar equipments developed during the War.

Those who worked under A. P. Rowe during the War have the greatest admiration for the way in which he inspired and led so large and varied a team. Many of his methods of scientific direction were novel; for example, his well-known 'Sunday Soviets', where high-ranking officers and scientific men frankly discussed the outstanding problems of the War. On the termination of hostilities, Mr. Rowe felt that his work at the Telecommunications Research Establishment had been completed, and after a short period with the Admiralty he went to Australia to advise on the construction of the rocket-firing range for the Ministry of Supply and the Australian Government. A. P. Rowe's chief interests are in the application of scientific methods to defence; the need for the British Empire to lead in this field might almost be said to be a passion with him. Australia is indeed fortunate in having such a man to guide a department to advise on the scientific aspects of defence.

British Journal of Nutrition

The need for a journal dealing with the scientific aspects of nutrition has long been felt in Great Britain, and, but for the War, its publication would have been one of the first preoccupations of the Nutrition Society. In war-time, however, the practical application of nutrition was deemed of more immediate importance, a circumstance which led to the founding of the Society's *Proceedings*, in which reports of its conferences on practical problems of nutrition are recorded *in extenso*. The field of nutrition occupies an important part of the domain of preventive medicine, and it was fitting that in launching this publication the Nutrition Society should have had the help and support of the Royal College of Physicians in the person of Lord Moran, its president. With a return to slightly more normal times, the Society has been able to realize its original wish. Arrangements have been made with the Cambridge University Press for the production of a quarterly journal, the *British Journal of Nutrition*, which will be devoted to the publication of original work in all branches of nutrition. The fifth volume of the *Proceedings*, recording conferences held during 1946, will be the last to be published separately; in the future, they will be merged in the new journal. Abstracts of communications made at ordinary scientific meetings, a new venture of the Society, will also appear in the *British Journal of Nutrition*. The editorial board will consist of D. P. Cuthbertson, J. N. Davidson, R. C. Garry, G. Graham, J. Hammond, L. J. Harris, E. M. Hume, S. K. Kon (chairman), I. Leitch, W. C. Miller, B. S. Platt, J. A. B. Smith, and F. Yates.

The first number of the new journal will probably appear before the end of the summer. Papers for publication and letters on editorial matters should be sent to Dr. S. K. Kon (*British Journal of Nutrition*), National Institute for Research in Dairying, Shinfield, nr. Reading, Berks. Subscriptions and inquiries about the *Journal* should be addressed to the Cambridge University Press, Bentley House, 200 Euston Road, London, N.W.1. The subscription rates for non-members of the Nutrition Society are £3 per volume of four quarterly parts, and separate parts will cost 20*s*. each.

Royal Society of New Zealand

The Sixth Science Congress under the auspices of the Royal Society of New Zealand was held during the week beginning May 18. This Congress was the first held since the outbreak of war, and it was attended by more than 700 members. In addition to the General Section, there were eleven sections covering practically every department of science; in these sections 220 papers were contributed. Prof. W. N. Benson's presidential address to the Congress was entitled "Scientists Look Toward the Future".

At the annual meeting of the Council of the Royal Society of New Zealand held on May 22, the following officers were elected: *President:* Dr. E. Marsden, secretary to the N.Z. Department of Scientific and Industrial Research; *Vice-President:* Dr. R. A. Falla, director of the Dominion Museum, Wellington. The following were elected fellows of the Society: Dr. M. A. F. Barnett, Government Meteorologist, Wellington; Dr. Ernest Beaglehole, lecturer in psychology, Victoria University College, Wellington; Miss Lucy B. Moore, botanist, Plant Research Bureau, Wellington; and Prof. H. G. Forder, professor of mathematics, Auckland University College, Auckland. The T. K. Sidey Summer-time Medal and Prize were awarded to Dr. D. F. Martyn, Commonwealth Observatory, Mt. Stromlo, Canberra.

Mond Nickel Fellowships

In February 1946, Sir William Griffiths, chairman and managing director, Mond Nickel Co., Ltd., invited the Institute of Metals, the Iron and Steel Institute, the Institution of Mining and Metallurgy, the Institute of British Foundrymen and the Institution of Metallurgists to co-operate with the Mond

Nickel Co., Ltd., in awarding annually for the next fifteen years a number of fellowships, to be known as 'Mond Nickel Fellowships'. The Company offered to make available over a number of years the sum of £50,000 to be used for awarding these fellowships. Particulars of the fellowships have now been issued. Mond Nickel Fellowships will be awarded to persons of British nationality educated to university degree or similar standard though they need not necessarily be qualified in metallurgy. There are no age limits, though awards will seldom be made to persons more than thirty-five years of age. Each fellowship will occupy one full working year; it is proposed to award up to five fellowships each year of an average value of £750. It is hoped by the aid of these fellowships to enable selected persons to pursue such training as will make them better capable of applying the results of research to the problems and processes of the British metallurgical and metal-using industries, and to increase the number of those who, if they are eventually employed in executive and administrative positions, will be competent to appreciate the technological significance of research and its results. The fellowships will be awarded to selected candidates who wish to undergo a programme of training in industrial establishments and will normally take the form of travelling fellowships; awards for training at universities may, however, be made in special circumstances. Forms of application can be obtained from the Secretary, Mond Nickel Fellowships Committee, 4 Grosvenor Gardens, London, S.W.1. Completed application forms must reach the Secretary of the Committee not later than September 1, 1947.

New York Academy of Medicine: Section on Microbiology

THE New York Academy of Medicine has decided to form a Section on Microbiology. This new Section, which brings the number of sections of the Academy to twelve, has for its main objectives the encouragement of the exchange of information among microbiologists, and the promotion of ready contacts between clinical and laboratory investigators. The fellowship of the Section will be broad and will include not only those who have a direct interest in microbiology, but also those who deal with microbiology in their primary functions as clinicians or scientists in other branches. The officers of the newly organised Section are: *Chairman*, Dr. Gregory Shwartzman, Mount Sinai Hospital, New York; *Secretary*, Dr. Harry Most, New York University College of Medicine; *Advisory Committee*, Dr. René J. Dubos, Rockefeller Institute for Medical Research; Dr. Frank L. Horsfall, jun., Rockefeller Institute for Medical Research; Dr. Colin M. MacLeod, New York University College of Medicine; Dr. Ralph S. Muckenfuss, Research Laboratories, New York City Health Department; and Dr. John G. Kidd, Cornell Medical College.

Invention of the Telephone

IN connexion with the centenary address on "Alexander Graham Bell and the Invention of the Telephone" given before the Royal Society of Edinburgh by Prof. G. W. O. Howe, an abridgment of which was published in *Nature* of April 5, Capt. G. C. Wilson, 375 McKay Street, Ottawa, has written maintaining that the telephone was invented at Brantford, Ontario, the Canadian home of the Bell family, and not at Boston. In support of this a quotation is given from a letter written by Bell in 1904, saying: "Now it so happens that the telephone was invented in Brantford during my visit to my father and mother in 1874". By 'invention' he probably meant the conception of the idea, for it was in the following February that he visited Henry in Washington and explained to him his idea of an articulate telephone. Henry encouraged him to develop the idea, which he thought might be the germ of a great invention. It was on June 2, 1875, that, while experimenting with the harmonic telegraph in Boston, the slight accident occurred which gave Bell the clue to the solution of the telephone problem, and as his assistant Watson said, "the speaking telephone was born at that moment". Where the invention was made depends on what one means by 'invention'. Bell certainly carried out experiments at his parents' home at Brantford when he was not at Boston; but it is generally agreed that it was in Boston that the experimental development occurred which can be regarded as the birth of the telephone.

Statue of Eros in Piccadilly Circus, London

APART from its interest to Londoners and its inherent æsthetic merit, the statue of Eros, surmounting the Shaftesbury Memorial Fountain in Piccadilly Circus, London, has a scientific aspect. Originally unveiled in 1893, it is cast in aluminium alloy, the basal structure being of bronze. Its restoration, after a war-time exile, necessitated a thorough cleaning, the repair of a few defects, and the provision of a new bow and arrow. The work was entrusted by the London County Council to Mr. G. Friese Greene, of Messrs. Starkie Gardner, Ltd., of Southfields, craftsmen in metals, since the original makers are no longer in business. The cleaning was carried out with neutral soap and warm water, the surprising discovery being that there is no trace of corrosion arising from the past fifty years in the London atmosphere; with suitable care the original oxide film of the aluminium and the *patina* of the bronze have both been retained. The statue is actually cast in pieces, and the latter are threaded over a steel tree and swaged together, the joints being difficult to find. The hot forging and the cold hammering of the new bow and arrow showed that aluminium alloy can be a very useful metal for sculptors and is actually easier to work than stone. The experience with the restoration of Eros should give an impetus to designers to make more use of these 'modern metals'.

Museum of the History of Science, Oxford

THE Museum of the History of Science, Oxford, whose collections, during the war period, were stored in the basement, is reopening on July 2 with an exhibition of historic scientific instruments pertaining to medicine and surgery. The exhibition will include the extremely fine collection of microscopes acquired from Dr. Reginald Clay in 1944 and now for the first time publicly exhibited. The immediate occasion of the exhibition is the meeting of the Association of Surgeons of Great Britain and Ireland and of the International Physiological Congress, both of which will be held in Oxford in July. The exhibition will be held in the School of Natural History, the ground floor room of the Old Ashmolean Building, which has not been open to the public during the past fifty

years. There will be a simultaneous exhibition of books and manuscripts illustrating medicine, surgery and physiology in the New Bodleian Library.

Lady Tata Memorial Trust Awards

THE Trustees of the Lady Tata Memorial Fund announce that, on the recommendation of the Scientific Advisory Committee in London, they have made awards as follow for research in blood diseases, with special reference to leukæmia, in the academic year beginning on October 1, 1947; the place at which the awards will be used is given in brackets: *Grants for research expenses and assistance:* Dr. Jørgen Bichel (Aarhus); Dr. Pierre Cazal (Montpellier); Dr. Pierre Dustin (Brussels); Dr. Peter A. Gorer (London); Dr. Maurice Guérin (Paris); Dr. Jozef Japa (Cracow); Dr. Edith Paterson (Manchester); Prof. Edoardo Storti (Pavia); Dr. Johannes Clemmesen (Copenhagen); and Dr. Tage Kemp (Copenhagen). *Scholarships (for whole-time or part-time research):* Dr. Simon Iversen (Copenhagen); Dr. C. F. M. Plum (Copenhagen); Dr. G. H. R. Tötterman (mainly in Helsinki).

The Night Sky in July

FULL moon occurs on July 3d. 10h. 38m. U.T., and new moon on July 18d. 04h. 15m. The following conjunctions with the moon take place: July 15d. 05h., Mars 0·9° S.; July 26d. 07h., Jupiter 0·2° S. In addition to these conjunctions with the moon, Mercury is in conjunction with Venus on July 22d. 09h., Mercury being 4·9° S. Mercury sets at 21h. at the beginning of July and is too close to the sun for favourable observation. The planet is in conjunction with the sun on July 14, and at the end of the month rises 1h. 20m. before sunrise. Venus is too close to the sun to be observed with advantage, rising about an hour before sunrise during the month. Mars, in the constellation of Taurus, rises at 1h. 30m., 1h. and 0h. 38m. at the beginning, middle and end of the month, respectively, and is a little fainter than a first magnitude star. Jupiter, in the constellation of Libra, can be seen in the earlier portion of the night, stellar magnitude about −2, setting at 1h. on July 1 and at 23h. on July 31. Saturn, in the constellation of Cancer, is too close to the sun for favourable observation, but can be seen for a short time at the beginning of July, when it sets one and a half hours after the sun. Occultations of stars brighter than magnitude 6 are as follows: July 13d. 01h. 44·9m., 31 Aries (*R*); July 27d. 22h. 12·1m., ρ Ophi. *m* (*D*). The latitude of Greenwich is assumed and *R* and *D* refer to reappearance and disappearance respectively. An occultation of Mars occurs on July 15d. 03h. 20·5m., reappearance taking place at 4h. 13·6m. The earth is in aphelion on July 5, its distance from the sun then being slightly more than 94½ million miles.

Announcements

DR. F. H. S. CURD, Dr. D. G. Davey and Dr. F. L. Rose, of Imperial Chemical Industries, Ltd., have been awarded the Gold Medal in Therapeutics of the Society of Apothecaries in recognition of their work in the preparation of the anti-malarial drug 'Paludrine'.

MR. D. J. B. COPP, University College, Southampton, has been appointed assistant secretary of the British Association as from July 1.

THE following appointments in the University of Leeds have been announced: Mr. G. P. Meredith, lecturer in educational psychology; Mr. H. Stansfield, lecturer in mechanical engineering; Mr. S. Mackey, lecturer in civil engineering; and Dr. E. Mundel, lecturer in electrical engineering.

THE following appointments have been made in the University of Sheffield: A. J. Hunt and R. A. G. Savigear, to be assistant lecturers in geography; A. E. Bender and Monica Lindsay, to be assistant lecturers in biochemistry.

THE following appointments, promotions and transfers in the Colonial Services have recently been made: H. W. Dougall, agricultural chemist, Sierra Leone/Gambia; N. Lawrence, professional assistant, Royal Observatory, Hong Kong; G. H. Payne, assistant government chemist, Tanganyika; W. J. Blackie (senior chemist, Fiji), deputy director of agriculture, Fiji; H. C. Thorpe (plant breeder, Kenya), senior plant breeder, Kenya; C. E. Duff (senior assistant conservator of forests, Northern Rhodesia), conservator of forests, Northern Rhodesia; R. G. Miller (assistant conservator of forests, Northern Rhodesia), senior assistant conservator of forests, Northern Rhodesia.

DR. L. S. B. LEAKEY states that a limited number of excellent casts of the mandible of the Lower Miocene ape *Proconsul* (see *Nature*, 152, 319; 1942) are now available for distribution, as well as a good cast of the large facial fragment. Those interested should write to the Curator, Coryndon Museum, P.O. Box 658, Nairobi.

THE response to the proposal to form a British Herpetological Society has been sufficient to enable further steps to be taken to inaugurate the society. An inaugural meeting will be held at the rooms of the Linnean Society, Burlington House, Piccadilly, London, W.1, on Friday, July 11, at 5 p.m. A report on the proceedings of the meeting will later be published for the benefit of members. All secretarial work in connexion with the formation of the Society is now being undertaken by Mr. Alfred Leutscher, of 5 St. Margaret's Court, London, E.12, as Capt. J. D. Romer is shortly to take up an appointment abroad.

THE second annual electronics exhibition, arranged by the Institution of Electronics (North Western Branch), will be held at the Manchester College of Technology on July 22 (2.30–9·0 p.m.) and July 23 (10 a.m.–9 p.m.). Two shows of scientific films will be given each evening. Admission is by ticket obtainable from A. Coates, 16 Didsbury Park, Manchester 20.

WE have received from the Murphy Chemical Co., Ltd., of Wheathampstead, Herts, a useful pamphlet of 32 pages on "Insecticides and Fungicides" supplied by them. It includes hints as to the proportions of the spray-mixtures to be used in various cases and a spraying programme for fruit and market garden crops.

ERRATUM. Dr. C. W. Shoppee has directed attention to the fact that Prof. T. Reichstein and Dr. L. H. Sarett have so far prepared only five of the adrenocortical hormones by partial synthesis, and not six as was stated in the article entitled "The Adrenal Cortex" (*Nature*, June 7, p. 765). The missing one is Reichstein's substance *M*, 17 (α)-hydroxycorticosterone.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

The Corpus Allatum and the Control of Metamorphosis in Insects

MOULTING in *Rhodnius* is initiated by a hormone secreted in the dorsum of the brain, and the premature metamorphosis of the young stages is prevented by a second hormone, the juvenile hormone secreted by the corpus allatum[1-3]. This conception of growth regulation in insects has proved applicable to Orthoptera[4-7], Lepidoptera[8-10] and Coleoptera[11]. In the Diptera, on the other hand, the composite 'ring gland' appears actively to induce metamorphosis, as though it were secreting a metamorphosis-promoting hormone[12].

There is, however, some evidence that the central part of the ring gland, the part representing the corpus allatum, restrains the differentiation of the imaginal disks in the early stages in *Drosophila*[13], and recently Vogt[14] has found that the corpus allatum of the adult *Drosophila* implanted into the larva will cause a local suppression of adult characters.

This last observation implies that the corpus allatum of the adult insect is once more secreting the juvenile hormone. In the grasshopper *Melanoplus*, Pfeiffer[6] has shown that such is the case; and she has even suggested that the yolk-forming hormone of the adult insect may prove to be identical with the juvenile hormone of the young stages.

I have recently found that this is so, apparently, in *Rhodnius*. The corpus allatum of the adult *Rhodnius*, implanted into a 5th stage nymph, causes it to develop into a giant 6th stage nymph; and the secretion from the corpus allatum of the 4th stage nymph will induce egg production in the decapitated adult female.

I have found also that there is a second factor in metamorphosis. The corpus allatum of the moulting 5th stage nymph in *Rhodnius* not only ceases to secrete the juvenile hormone, but also it actively removes from the blood any traces of the juvenile hormone that remain. The decapitated 4th stage nymph exposed to the moulting hormone from the 5th stage nymph undergoes a precocious metamorphosis; but the adult characters are differentiated more completely if the 5th stage nymph retains its corpus allatum. The corpus allatum of the 5th stage nymph in the late stages of moulting or of the newly moulted adult, implanted into intact 3rd or 4th stage nymphs, reduces the activity of the juvenile hormone so that when they reach the 5th stage some of them show a partial metamorphosis (Figs. 1 and 2).

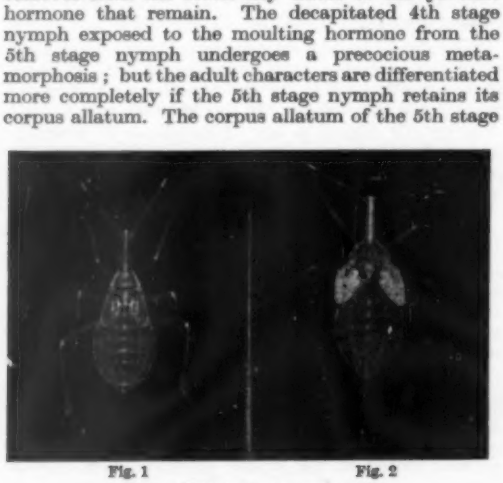

Fig. 1 Fig. 2

Fig. 1. NORMAL 5TH STAGE NYMPH

Fig. 2. 5TH STAGE NYMPH FOLLOWING IMPLANTATION OF CORPUS ALLATUM OF YOUNG ADULT INTO THE 3RD STAGE, SHOWING PARTIAL METAMORPHOSIS

This effect is always diffused throughout the body; it is never localized in the region of the implant, as the effect of the juvenile hormone often is. It is unlikely, therefore, that it is due to the secretion of an imaginal or metamorphosis hormone.

I suggest that it may be the active elimination of juvenile hormone in the Diptera which creates the impression of a 'metamorphosis-promoting hormone'. If it be supposed that these two factors, the cessation of secretion of the juvenile hormone and the active elimination of this hormone, differ in their relative importance in different insects, we may perhaps have the basis for a theory of metamorphosis which will be applicable to all groups of insects.

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Evolutionary Significance of Periodicity of Variation-Intensity and Population-Flux in the Desert Locust

THE periodic mass-increase in the population of the desert locust, *Schistocerca gregaria* (Forskål), which ranges from Africa to India, is a phenomenon known since Biblical times. The compilation of historical records of the last century and a half has established an *average* 11-year cycle of mass-increase, although the individual periods are far less regular. Following Uvarov's work[1,2], the existence of two phases, namely, the swarming or *gregaria* phase and the non-swarming or *solitaria* phase, has also been established. These phases differ morphologically and in other ways, are connected by a graded series of intermediates, and are evidently of no genetical significance; they can be produced at will by the simple process of crowding and thinning.

The last cycle of increase lasted from about 1926 until 1931, followed by a *solitaria* period of several years. We are now in the middle of another cycle which started in 1940. As a result of considerable work in various countries during the last fifteen years, we now possess a fairly comprehensive knowledge of the migrations and population-flux of this species. Briefly, the species in *solitaria*-years is confined to certain desert tracts extending from West Africa via the Red Sea coast to the Persian coast and Western India, including Rajputana; the population is low, usually not exceeding about 1,000 per sq. mile, and often much less. In *gregaria*-years the population increases enormously (considerably more than 10,000 per sq. mile) and the range also increases, the species spreading to Southern Europe (extending even to

Great Britain in some years), Transcaucasia, Central Asia, Peninsular India and Eastern India (Assam).

I have already shown that during the *solitaria* phase the species is polymorphic in regard to the number of eye-stripes, the wild populations consisting of two main types (6- and 7-eye-striped), with 8-striped forms turning up occasionally in extreme *solitaria*-years (1936–38). During the *gregaria* period, that is, in swarms, *only* 6-striped individuals occur, and the eyes are more heavily pigmented. Occasionally, 5-striped individuals occur in laboratory rearings, evidently as mutants, but such individuals have not yet been found wild. I have elucidated[3,4] the pigmentary basis of stripe formation and their development. The eye-stripe polymorphism is partially correlated with the number of moults[4] and also with the number of antennal segments[5]. In addition to and superimposed upon these variations, two types of colour variations occur in the *solitaria* phase: the majority of wild individuals (91 per cent) are blue-grey as adults and green as hoppers, while the remainder are fawn as adults and buff as hoppers[6,7].

The eye-stripe polymorphism was noted in India and North Africa during the *solitaria* period of 1932–39. It was absent during the past swarming periods, as judged from the examination of museum specimens. During the present swarming cycle (1940–) the polymorphism has again practically disappeared, only a few 7-striped forms being found in North-West India during 1940–46 *after* the main swarms had left; no 8-striped forms have yet been found (Y. R. Rao, *in litteris*).

From the foregoing account, the following important conclusions emerge: (i) The population fluctuates periodically, the 'plague period' of high numbers corresponding with a considerable increase of range of the species. (ii) As regards variation intensity, the species is comparatively stable during the plague or *gregaria* period but highly polymorphic during the *solitaria* period when the population is low. (iii) The phenomenon is periodic and evidently occurs over a wide range (India to Africa) more or less simultaneously. (iv) It would appear that the 7- and 8-striped individuals, which make their appearance during *solitaria* periods when selection-pressure is low probably owing to reduced numbers, are eliminated, presumably by selection, during the *gregaria* periods of high population and increased selection-pressure.

Crossing results show that 6-striped individuals produce both 6- and 7-striped forms in F_1 and F_2 generations. Other results need not be discussed here for the present.

The evolutionary consequences of these phenomena lie in the fact that we have here probably the first clear case of the Wright – Dubinin effect (of random or automatic increase of variation intensity in small, partially isolated populations; random 'drift' of Wright[8]; genetico-automatic processes of Dubinin[9]) in natural populations showing periodic fluctuations. As, fortunately, the periods are small (average eleven years), it is possible to test the results repeatedly and even calculate the rate of flow of a particular variation in Nature[10].

It is interesting that the opposite effect, namely, an increase of variation intensity with increased abundance, is also known to occur[11–13], and Fisher has even provided the mathematical basis of it. The question whether these two opposite effects are due either to totally different mechanisms, or are merely the different expressions of fundamentally the same mechanism, cannot be discussed here, but must be borne in mind. The Hagedoorn effect[14] of steady random (automatic) decay of variation intensity in the total absence of selection has not been demonstrated in Nature; it has been criticized on mathematical grounds by Fisher as being too slow to be ever of any significance in evolution.

A fuller account will be published elsewhere.

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A Tubal Factor Concerned in the Denudation of Rabbit Ova

McClean and Rowlands[1] found that hyaluronidases from several sources were able to liquefy the viscous gel cementing the follicle cells around freshly ovulated rat ova, thereby denuding the latter. This was confirmed by Fekete and Duran-Reynals[2] for the mouse and by Leonard and Kurzrok[3] for the rat. On the basis of their observations, McClean and Rowlands suggested that the gel consisted of hyaluronic acid, and that the function of seminal hyaluronidase was to bring about denudation of the egg prior to fertilization. Pincus[4] had noticed that although rabbit cumuli placed in rabbit sperm suspensions underwent rapid disintegration, quite frequently a layer of more densely packed follicle cells (corona radiata) remained surrounding the egg. On the other hand, when this action took place inside the Fallopian tube, the ova became completely denuded within 1½–3 hours of ovulation if insemination had previously occurred, or within 6–8 hours if the mating had been with a vasectomized buck.

Since, in an investigation of the action of hyaluronidase on rabbit cumuli *in vitro*, it had been observed that the corona radiata appeared invariably to remain intact, it was decided to study more closely what part was played by the Fallopian tube in bringing about complete denudation of rabbits' ova.

Female Dutch rabbits were injected intravenously each with 50 I.U. of chorionic gonadotrophin to induce ovulation. Twelve hours later they were killed by air embolism and the Fallopian tubes were removed and washed through with physiological saline to recover the ova, which, in these circumstances, were always embedded within cumuli. The cumuli were placed in fresh saline to remove any material that might have been washed from the tubes, and divided so as to contain two ova each. The portions of cumulus were then placed in separate cells in the minimum amount of fluid. Two ejaculates of rabbit semen were suspended in 20 ml. of Baker's solution[5],

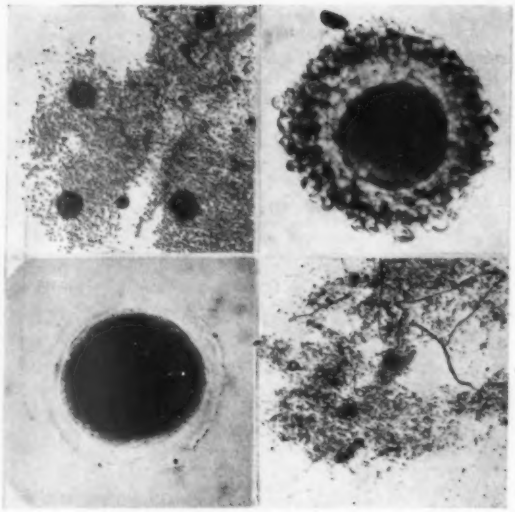

Fig. 1. Untreated cumulus containing three ova. (× 15)

Fig. 2. Ovum surrounded by corona radiata, after action of hyaluronidase on cumulus. (× 145)

Fig. 3. Completely denuded ovum, recovered from Fallopian tube into which it had been inserted while in the condition shown in Fig. 2. (× 145)

Fig. 4. Cumulus containing two ova, recovered from Fallopian tube into which it had been inserted while in the condition shown in Fig. 1. (× 15)

the suspension then being divided into two portions both of which were centrifuged for 10 min. The supernatant from one portion (*A*) was removed and fresh Baker's solution added to the original level, the sperms being stirred up. The sperms in the second portion (*B*) were stirred up in their original supernatant. 0·2 ml. aliquots of the three fluids (*A*, *A* supernatant, and *B*), the sperms being fully active in *A* and *B*, were then added to the cumuli in their cells, each fluid being added to two cumuli. The times required to free the eggs from their cumuli are shown in the table.

Fluid	Time required to free ova (min.)	Average time (min.)
A	21	26
A	31	
A supernatant	20	
A supernatant	26	23
B	20	20
B	20	

In all the above the coronæ remained intact, and it is clear that the presence of spermatozoa is unnecessary for disintegrating the cumulus, hyaluronidase in solution being adequate for the purpose. Thirty hours later the coronæ were still intact. The addition of a tubal extract, made by grinding two Fallopian tubes with sand, extracting with saline and filtering, was also without effect on the corona, neither did the addition of 10 units of standard freeze-dried bull semen filtrate (Swyer and Emmens[6]) produce any effect within 1½ hours.

In the next series of experiments, cumuli were obtained as before from three rabbits, some being placed in a solution consisting of 10 units of hyaluronidase (bull) in 2 ml. saline which freed the eggs within 10 min., leaving the coronæ intact. Under ether anæsthesia the abdomen was opened in three other rabbits, and three or four eggs treated as above were inserted into the fimbriated end of one tube while an untreated cumulus was inserted into the other in each rabbit. The wounds were closed in the usual way. Three hours later the rabbits were killed, the tubes washed out and the eggs recovered. The cumuli which had been inserted intact were recovered intact, while the treated eggs were recovered either completely denuded or with only a few adherent follicle cells (Figs. 1–4).

It is clear, therefore, from these results that disintegration of the corona radiata of the rabbit's ovum is effected, not by the action of hyaluronidase, but by some influence exerted by the Fallopian tube. This influence comes into play after the cumulus has been removed, ordinarily by the action of hyaluronidase, but in the absence of insemination, by the much slower action of the tube itself, as shown by the time relations established by Pincus. The nature of the tubal factor is not clear at present; the fact that tubal extracts are without action on the corona or cumulus would seem to rule out a purely chemical effect, and perhaps suggest a mainly mechanical one dependent, for example, on ciliary action and muscular contraction. Attempts at demonstrating such an action have been made by inserting hyaluronidase-treated ova into the tubes of pseudo-pregnant rabbits (in which tubal activity, as a result of the influence of progesterone, might be considered as diminished) and also by instilling 0·2 per cent procaine along with the treated ova into the tubes of normal rabbits. In all these experiments the recovered ova were denuded of their coronæ, so that no conclusions could be drawn from them.

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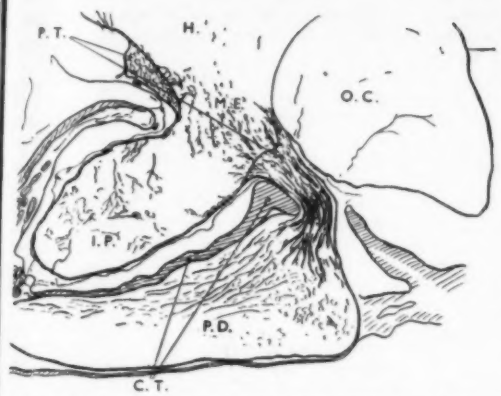
The Hypophysial Portal Vessels of the Porpoise (*Phocæna phocæna*)

If the nervous system influences the activity of the anterior lobe of the pituitary gland by liberating some humoral excitant into the hypophysial portal vessels, which in turn transmit the substance to the gland[1,2], then it would seem likely that these portal vessels would be widely distributed among the vertebrates. It is well known that in birds and some mammals (whale, sea-cow, Indian elephant, armadillo and porpoise) the neural lobe of the pituitary is separated from the pars distalis of the anterior lobe by a connective tissue septum, and it seemed possible that this septum might interrupt the path of the portal vessels in these animals.

To investigate this possibility a male porpoise (*Phocæna phocæna*) weighing 136 lb., and 5 ft. in length, was injected in a cranial direction through the vertebral arteries with India ink. After completing fixation, the hypothalamus, pituitary and surrounding structures were taken *en bloc*, serially sectioned in a sagittal plane at 150 μ, and the sections stained with Biebrich scarlet. The sections show clearly that the porpoise possesses a hypophysial portal system of blood vessels similar to that of other mammals. The pars tuberalis extends upwards to embrace the tuber cinereum as a ring-like collar. The portal vessels start as the characteristic sinusoidal



MID-LINE SAGITTAL SECTION THROUGH THE HYPOTHALAMUS AND PITUITARY GLAND OF A PORPOISE. VESSELS INJECTED WITH INDIA INK. CAMERA LUCIDA DRAWING. × 9·3

C.T., connective tissue septum, derived from the dural capsule of the gland, intervening between the infundibular process and the pars distalis; *H.*, hypothalamus; *I.P.*, infundibular process; *M.E.*, median eminence containing the characteristic vascular loops and tufts; *O.C.*, optic chiasma; *P.D.*, pars distalis; *P.T.*, pars tuberalis

loops in the median eminence, drain into the vascular plexus of the pars tuberalis, where they unite to form the widely dilated trunks which pass ventrally into the sinusoids of the pars distalis. The detailed arrangement is very similar to that of the rabbit, in that the trunks of the vessels are contained in a thick pillar of cells (termed the zona tuberalis in the rabbit[2]), which passes through the diaphragma sellæ by a foramen separate from that transmitting the infundibular stem.

Thus the pituitary gland of the porpoise, in spite of the septum intervening between the two lobes, conforms with that of other mammals in possessing the typical vascular connexions between the median eminence and pars distalis. It is of interest that Drager[4,5] finds the pars distalis of the bird and porpoise to be free of nerve fibres.

I wish to thank Mr. D. A. Parry, Department of Zoology, Cambridge, for the porpoise.

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Cytosiderosis and the Genesis of Arteriosclerosis and other Fibrous-Tissue Reactions

CYTOSIDEROSIS is a diffuse metabolic disorder characterized by the deposition of large quantities of iron pigment, at first mainly in the liver and later in other organs and tissues of the body[1,2]. It occurs consistently in adult pellagrins, although it is commonly seen also in other members of the African population not suffering overtly from acute episodes of malnutrition[3].

Serial liver biopsies of the same pellagrin have revealed that the iron pigment is lodged first in the hepatic parenchyma and then later in the reticulo-endothelial system in the liver. By piecing together the evidence derived from our biochemical and pathological studies, we were able to show that the second stage of the disease is characterized by a true siderosis whereby the iron pigment is steadily disseminated via the gut mesenteric glands into the thoracic duct and eventually by the blood circulation[4]. The absorption of such large molecules presupposes a disturbance in digestion and in the absorptive capacity of the gut epithelium.

Obstruction to the pancreatic duct of cats fed a particular diet containing large quantities of iron over a period of time results in the appearance of excessive amounts of iron in the liver[5,6]. This was attributed to a vitamin A deficiency which impaired the absorptive efficiency of the gut epithelium[6]. Our own investigations in cats have shown that when the pancreatic ducts are tied, iron can still appear in the liver when vitamin A is being actively absorbed. Severance of the pancreatic duct and a high iron-containing diet greatly expedite this extraordinary disturbance in iron metabolism in cats. Normal cats fed the same diet only develop an iron flush in the Kupffer cells, whereas, over the same period of time, cats with obstructed pancreatic ducts accumulate massive quantities of iron pigment in the liver. From these experiments in cats, it is evident that a badly balanced diet can lead to the accumulation of iron-containing molecules in the reticulo-endothelial system.

It has been shown in our laboratory that the pigment in human cytosiderosis is a relatively inert molecule. The intravenous injection of this pigment into normal cats results in the accumulation of large masses of pigment, at first in the reticulo-endothelial system in the form of coarse clumps, and later in the liver cells situated near the medium-sized portal tracts, such as we have described in the second stage of cytosiderosis in African pellagrins. Injection of colloidal iron in amounts equivalent to the iron present in the pigment over the same period leads only to a slight flushing of the Kupffer cells similar to that noted in intact cats fed the equivalent of approximately 40 mgm. of iron per diem.

Whether or not the second stage of cytosiderosis in the African is associated in particular with a disorder of the pancreas is not of immediate consequence, although we have shown that in malnourished infants the early stages in the genesis of cystic fibrosis can be detected. The important issue, so far as this communication is concerned, resides in the observation that sooner or later in cytosiderosis an inert molecule first found intracellularly in the liver is excreted via the bile, then absorbed by the gut and disseminated by the systemic circulation. The flooding of the circulation with macromolecules can result in a number of extraordinary reactions constituting the macromolecular syndrome[7]. The noteworthy features of this syndrome are the production of arteriosclerosis, fibrosis of the spleen and curious hæmatological reactions. While the experiments conducted on the macromolecular syndrome by Hueper and others were mainly of an acute character, nevertheless the conclusions are unequivocal.

If now it is remembered that cytosiderosis in the African occurs very frequently in the third, and can be very extensive in the fourth decade, it is evident that a large, inert, iron-containing molecule is steadily flooding the circulation in minute amounts for ten to fifteen years. Such a state of affairs could quite easily lead to those conditions optimum for the development of premature arteriosclerosis and other instabilities of the cardiovascular system known

to occur in young Africans[8,9], in addition to cirrhosis of the liver, the tendency to keloids[10], reticulosis, etc. For such reactions to occur, it would appear that chronic malnutrition associated with disease of the liver and the pancreas might also be essential attributes of the background of the African.

While in African pellagrins we were able to track the movements of the macromolecule by virtue of its iron-containing fraction, it is also very likely that other types of macromolecules—protein, polysaccharides, lipoproteins and the like—may enter the circulation in conditions similar to those described for cytosiderosis. A new approach is now available for attacking the several problems connected with the tendency to fibrous tissue reactions noted in many metabolic disorders, and expressed as arteriosclerosis, hypertension, reticulosis and cirrhosis. Investigations are proceeding actively in this laboratory to elucidate this and other aspects of the problem.

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Hydrochloric Acid Formation in the Gastric Mucosa

IN a recent article[1] concerning the mechanism of high acid production by bakers' yeast and its bearing on hydrochloric acid formation in the stomach, reference was made by us to the work of Davies *et al.*[2,3] on the respiratory exchange and hydrochloric acid production by the gastric mucosa of the frog. It was pointed out that though such results appear of value for the theory of formation of hydrochloric acid, they in no way disprove the *essentials* of the theory proposed from our Department as to the manner of formation of the acid[4]. After reading the recent communication of Davies *et al.*[5], we can only reaffirm our conclusions in this respect concerning their data.

In their communication, and referring presumably to their findings that the maximum Q_{HCl} is twice the maximum Q_{O_2} for the isolated gastric mucosæ, they say that these "facts rule out theories dependent on the production of hydrogen ions by the fermentation or oxidative degradation of any carbon compound". Also, they write that a statement of ours to the effect that a cyclical process (with H ions deriving from an organic acid) could give a Q_{HCl} far higher than Q_{O_2} "is not justified".

Both these statements of Davies *et al.*[5] may be shown to be incorrect, as indicated in the accompanying cyclical scheme. In this the hydrogen ions are assumed to come from phosphoglyceric acid, produced by the dehydrogenation of triosephosphate; but, *it is to be understood that this reaction is chosen only for illustration*, and that some other reaction, or indeed several reactions, involving acidic production may be operative. The scheme is presented in stages which are numbered for convenience of reference. Some obvious connecting links have been omitted. For ease of presentation, single molecules or ions appearing in the reactions or exchanges have been given.

In the scheme, fluid containing potassium chloride in strength approximately isotonic with blood is excreted in a special region and flows past the acid region, potassium ions being exchanged for hydrogen ions. The hydrogen atoms which wander from the triosephosphate are received by an acceptor, but are not further transferred in this region of the cell to molecular oxygen.

In the fifth stage, the reversal of the dehydrogenation occurs—in another part of the cell—hydrogen ions now disappearing from solution and an alkalinity corresponding to the acid secreted being produced. In stage 6, the view of Davies *et al.*[5] that carbon dioxide *from the blood* neutralizes the hydroxyl ions and forms HCO_3 ions (passing via H_2CO_3, the formation of the carbonic acid being catalysed by carbonic anhydrase), is included, as this appears now to interpret well the final exchanges; but, as stated in our article[1], any view assuming carbonic anhydrase to act at the immediate site of the acidic formation would appear improbable. In stage 7, HCO_3 ions exchange for chlorine ions across the membrane.

A cyclical scheme of the above type could also be developed for other views such as those of Bull and Gray[6], which turn on the formation of an organic acid; and it would appear, too, that carbonic acid as a source of hydrogen ions could be similarly developed. The scheme presented is perhaps best described as a working hypothesis (and present views of hydrochloric acid formation in the stomach can be regarded as little more), but its most essential feature can be illustrated by the biological model of bakers' yeast working under certain conditions and producing a high degree of acidity.

Considering now the quotient Q_{HCl}/Q_{O_2}, which could result from such a scheme, it will be assumed for illustration that the phosphoglyceraldehyde and phosphoglycerate are shifted from a 9 : 1 to a 1 : 9 ratio (and then restored to the former ratio), and that for both conditions the equation:

CYCLICAL SCHEME FOR THE PRODUCTION OF HYDROCHLORIC ACID

	Lumen	Intracellular substance	Blood or intercellular spaces
1.		$R.CHO + \overset{+}{K} + \overset{-}{Cl} + Ac$	$\overset{-}{Cl} + CO_2 + H_2O$
2.	$\overset{+}{K} + \overset{-}{Cl}$	$R.CHO + Ac$	$\overset{-}{Cl} + CO_2 + H_2O$
3.	$\overset{+}{K} + \overset{-}{Cl}$	$\overset{+}{H} + R.\overset{-}{COO} + AcH_2$	$\overset{-}{Cl} + CO_2 + H_2O$
4.	$\overset{+}{H} + \overset{-}{Cl}$	$\overset{+}{K} + R.\overset{-}{COO} + AcH_2$	$\overset{-}{Cl} + CO_2 + H_2O$
5.	$\overset{+}{H} + \overset{-}{Cl}$	$R.CHO + \overset{+}{K} + \overset{-}{OH} + Ac$	$\overset{-}{Cl} + CO_2 + H_2O$
6.	$\overset{+}{H} + \overset{-}{Cl}$	$R.CHO + \overset{+}{K} + \overset{-}{HCO_3} + Ac$	$\overset{-}{Cl}$
7.	$\overset{+}{H} + \overset{-}{Cl}$	$R.CHO + \overset{+}{K} + \overset{-}{Cl} + Ac$	$\overset{-}{HCO_3}$

$$E_h = E'_0 - \frac{RT}{nF} \ln (\text{Red.})/(\text{Ox.})$$

applies with the same E'_0 value; then the free energy requirement per equivalent of hydrogen ions produced, to restore the condition as in the scheme, is approximately 1,700 cal., the free energy change per mol. oxygen used in oxidizing carbohydrate being about seventy times this value[7]. For the interpretation of the Q_{HCl}/Q_{O_2} values found by Davies *et al.*[5], this allows a very wide latitude for efficiency considerations, etc.

The views of Davies *et al.*[5,8] with respect to electric force playing an important part in the elaboration of hydrochloric acid, by way of splitting water molecules, we find unsatisfactory chiefly for the following reasons: (1) Their hypothesis appears to arise mainly from the mistaken view that the facts of the respiratory exchange preclude an explanation from the formation of hydrogen ions by organic acid. (2) No indication is given as to the manner in which such electric power operates. Other objections could be also advanced.

We regret having misinterpreted a statement in one of the summaries of Davies[3]. From evidence at our disposal, we concluded that Q_{HCl}/Q_{O_2} figures greater than 2·0 had been obtained, so that the sentence "In bicarbonate saline the Q_{HCl} sometimes rose to twice the Q_{O_2} without causing visible damage to the tissue" seemed to imply that beyond a Q_{HCl}/Q_{O_2} of twice, the tissue was damaged. We are glad that this point has been cleared up (but for the general theoretical interpretation it is of minor significance); and also that a printer's error in our article has now been corrected by Davies *et al.*[5].

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Volume Change Accompanying the Splitting of Ribonucleic Acid by Ribonuclease

A previous investigation by Linderstrøm-Lang and Jacobsen[1] has shown that dilatometric measurements can be useful in the study of the enzymatic breakdown of macro-molecules. Thus it was found that the contraction accompanying the tryptic and chymotryptic splitting of lactoglobulin was too great to be compatible with the assumption that the degradation of this protein is exclusively caused by an opening of peptide bonds in a rolled-up peptide chain. A recent extension of these experiments to include other proteins has made it likely that this phenomenon is common to the molecules of globular proteins, and that it is the breakdown of the superstructure of these molecules that causes the abnormal contraction.

We have thought it of interest, in analogy with these investigations, to study the breakdown of another type of macro-molecule, the nucleic acids. We have chosen ribonucleic acid because one of the authors (Chantrenne) was familiar with this substance from previous work. The particular ribonucleic acid preparation used was made according to Chantrenne (ref. 2, fraction A), but from Danish bakers' yeast. The product obtained was somewhat different from that previously described[2], and we therefore give the titration curves (Fig. 1) for the Danish nucleic acid before and after degradation with ribonuclease (prepared according to Kunitz[3]).

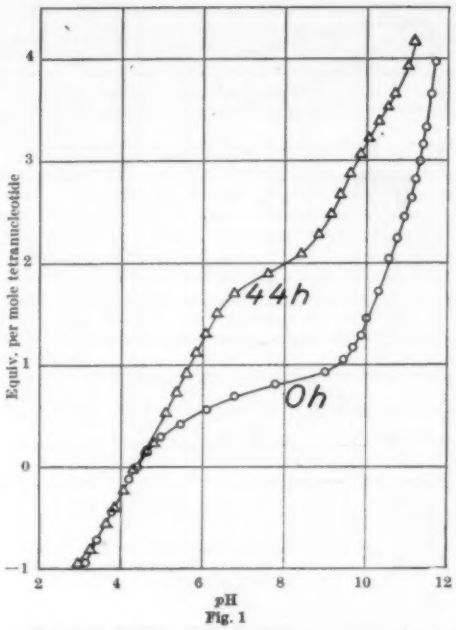

Fig. 1

It will be observed that the change in *p*H caused by the action of ribonuclease is negligible around *p*H 4·6 and below, and we can, therefore, feel confident that the volume change observed during an enzymatic process at this *p*H does not originate in an interchange of protons between the nucleic acid and the medium.

The following experiments may serve as an example of our results. *Enzymatic system.* Sodium salt of ribonucleic acid 1 per cent, *p*H 4·6, sodium chloride 0·1 *N*, ribonuclease 0·0125 and 0·005 per cent (curves I and II respectively), $T = 30°$. Dilatometric technique as described in ref. 1, but reduced to micro scale. *Determination of the opening of phosphate bonds:* microtitration with sodium hydroxide, end-point *p*H 8, indicator cresol red. Due to the continued action of ribonuclease at the end-point, extrapolation to zero titration time was made by means of titration-time curves.

The results are seen in Figs. 2 and 3. ΔP denotes the number of phosphate bonds opened, ΔV the volume change. The data are calculated as ml. and equivalents per mole tetranucleotide.

It will appear from the graphs that our results are compatible with earlier work on the action of ribonuclease[4–7], namely, that this enzyme has two functions, one as a 'depolymerase' or 'denaturase' and the other as a 'phosphatase'. In its first capacity it acts very rapidly, presumably preparing the nucleic acid molecule for the slower attack by the phos-

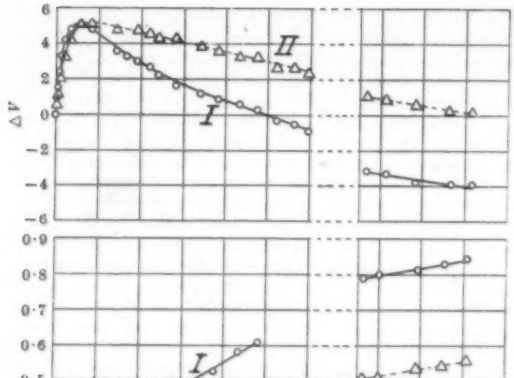

Fig. 2

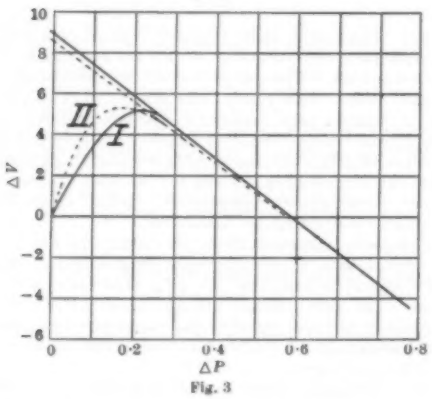

Fig. 3

phatase which breaks the molecule down to tetranucleotides. The 'depolymerization' is accompanied by an increase, the splitting of the phosphate linkages by a decrease, in volume. If we consider the right-hand part of the curves in Fig. 3, where the cleavage of the phosphate linkages is predominant, we observe that they are straight lines with a slope $\Delta V/\Delta P$ of about -13 ml./equiv. Assuming now that this relation holds true for the phosphate-bond cleavage during the entire enzymatic process, we obtain the total volume increase due to 'depolymerization' by extending the lines until they intersect the axis of ordinates. In this way we find a value of 8–9 ml. per mole tetranucleotide, or about one per cent of the nucleic acid molecule. We shall not discuss the significance of this rather hypothetical value here, but we would like to point out the similarity of the problem to that met with in the case of proteins. In both cases an initial process involving a breakdown of some superstructure seems to be required before the 'true' chemical degradation can set in. We cannot offer any satisfactory explanation why this process is accompanied by a contraction of the protein and a dilatation of the nucleic acid, but we are willing to follow Gulland and his collaborators in assuming that some kind of 'physical' bond between the purines and pyrimidines is present and put out of action in the latter case. The possibility of resonance is considerable in these substances. But it must be borne in mind that the opening of bonds involving ionogen groups (see ref. 8) is unlikely, due to the constancy of *p*H.

Finally, we would point out that the value $\Delta V/\Delta P = -13$ is surprisingly high for the splitting of an ester bond when, as in the present case, the hydroxyl group belonging to the phosphoric acid remains uncharged. In the splitting of glycerophosphate by acid phosphatase at *p*H 4·6, the corresponding value is about -3 ml./equiv. This question requires further investigation.

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Effect of Ultra-Filtration and Carbon Dioxide on the Prothrombin Activity of Plasma

It is well known that the prothrombin in plasma is easily susceptible to inactivation, but the nature of the factors responsible for this inactivation has not been clearly understood. M. Gerendas[1] has stated that the inactivation of thrombin is due to two parallel and separate causes, namely, adsorption, and a fermentative inactivating process.

Investigations carried out in this laboratory on the nature of the factors responsible for the inactivation of plasma prothrombin have shown that the prothrombin activity of plasma which has been inactivated on keeping can be regained either by ultra-filtration or by passing carbon dioxide through the plasma; hence it is reversible. However, if the plasma is stored for a longer period (more than a week), the prothrombin undergoes irreversible inactivation, and in such cases neither ultra-filtration nor carbon dioxide has any effect. It should be noted, however, that neither carbon dioxide nor ultra-filtration has any effect on the prothrombin activity of fresh plasma. We have also observed that in the case of inactivated plasma, on adding the ultra-filtrate to the ultra-filtered plasma, the prothrombin is inactivated. These experiments clearly show that the inactivation of prothrombin is due to the presence or formation of substances which can be removed by ultra-filtration.

While studying the prothrombin activity in pathological conditions, such as tuberculosis and jaundice, we found that the low prothrombin activity in such cases can invariably be increased by ultra-filtration or by passing carbon dioxide through the plasma.

In the following table are given some of the typical results of our experiments.

Coagulation time (in sec.)

Plasma	Original plasma	After ultra-filtration	After passing CO_2 for 30 sec.	Prothrombin activity of 0·2 c.c. ultra-filtered plasma + 0·2 c.c. thrombo-plastin	
				+0·2 c.c. water	+0·2 c.c. ultra-filtrate
Bovine plasma					
Fresh	14	—	14	—	—
	19	—	19	—	—
After 24 hr.	30	22	20	—	—
After 48 hr.	28	20	—	21	51
After 72 hr.	42	24	—	27	64
Human plasma (normal)					
Fresh	12	—	13	—	—
After 5 hr.	44	17	—	20	28
Human plasma (pathological)					
Jaundice					
Fresh	15	—	10	—	—
Tuberculosis					
(a) Pulmonary					
Fresh	21	—	—	—	—
After 8 hr.	35	20	14	25	32
Fresh	No clotting until 420 sec.	—	46	—	—
After 4 hr.	"	17	47	—	—
Fresh	17	—	13	—	—
After 3 hr.	25	—	13	—	—
(b) Miliary					
	No clotting until 600 sec.	—	105	—	—

These results show an interesting parallelism between the effect of ultra-filtration and carbon dioxide on the prothrombin activity of plasma.

The mechanism of the effect of carbon dioxide on prothrombin activity of plasma is under investigation.

Our thanks are due to Prof. V. Subrahmanyan for his interest in these investigations.

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Influence of Cations in the Production of Citric Acid by Yeast

Succinic and citric acids were found by Wieland and Sonderhoff[1] to be products of the dissimilation of acetic acid by yeast under aerobic conditions. But whereas succinic acid was always obtained in a yield of 5 per cent, the yield of citric acid varied; it amounted to 10 per cent only when they used barium acetate.

Sonderhoff and Deffner[2] re-investigated the production of citric acid in a more systematic manner. They found a yield of 25 per cent under certain conditions by using barium acetate. They suggested, therefore, that this yield of citric abid could be explained by the insolubility of barium citrate. The insoluble barium citrate remains unaffected while sodium citrate, which is very soluble in water, can be easily oxidized further; and it is then easy to understand why traces only of citric acid are found by oxidizing sodium acetate.

Virtanen and Sundman[3] indicated that production of citric acid takes place by oxidizing magnesium acetate, although magnesium citrate is soluble in water. They did not give a complete explanation, but they assumed that magnesium and barium probably have an influence at some stage in the formation of citric acid.

In a series of experiments, we were able to demonstrate that by using cations forming easily soluble citric salts (sodium, potassium and lithium), almost no citric acid was found. We think that the citric salts of these cations can pass easily through the cell wall, and they are dissimilated in the interior. In presence of cations forming insoluble salts (lanthanum, barium and strontium) we obtained a yield of roughly 15 per cent citric acid, because the insoluble citric salts cannot penetrate to the interior of the cell.

In the case of magnesium we found that complex salts are formed. The complex anions have a big volume and they cannot pass through the cell wall. In the case of barium citrate, difficulty in penetrating the cell wall is due to the insolubility of the salt; in the case of magnesium citrate to formation of a complex.

By using ordinary yeast, we found that the citric salts of sodium and potassium are easily dissimilated; those of magnesium and barium scarcely at all. By treating yeast with liquid air[4] and thus destroying the cell wall, we found equal dissimilation of sodium, magnesium, calcium and barium citrates. However, by this method of using yeast with destroyed cell wall, we were not able to oxidize acetic salts.

A detailed account of the experiments will be published elsewhere as soon as the investigation is completed.

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Slow Cosmic Ray Mesons at Sea-Level

During the latter part of 1938, a high-pressure cloud chamber designed by the late Prof. E. J. Williams was constructed to investigate certain phenomena concerned with cosmic ray mesons. It was hoped that by using high pressures, giving a long sensitive-time, that mesons would be observed coming to the end of their range in the cloud chamber. Four such examples have been obtained[1]. A similar example was also found by E. J. Williams and G. E. Roberts using a large cloud chamber working at atmospheric pressure[2]. In each case, the meson is observed to come to rest and then to decay with the emission of a β-particle. From these results, it is estimated that the proportion of slow mesons ($\beta < 0{\cdot}25$) in cosmic radiation at sea-level is about 0·05 per cent. In these experiments, no evidence has been found of the disintegration of nuclei by mesons as reported recently by D. H. Perkins[3], and by C. F. Powell and G. P. S. Occhialini[4]. Little information concerning the frequency of such events at sea-level is available, and it was thought that it would be worth while to expose some photographic emulsions in the same room as the cloud chamber.

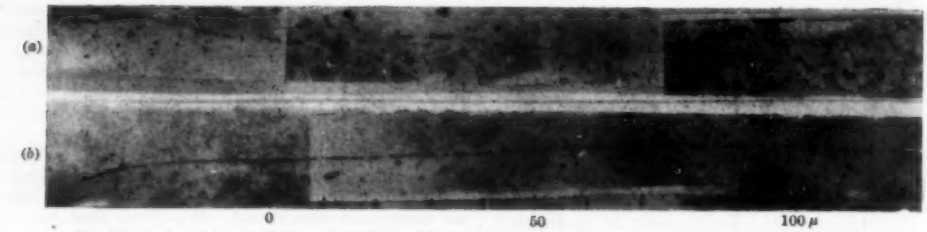

Fig. 1. (a) TRACK OF A MESON OF MASS 250 ± 50 m_e COMING TO REST IN THE EMULSION. (b) TRACK OF A PROTON FOUND ON THE SAME PLATE

Unfortunately, this work was interrupted by the War and only resumed a few months ago.

We have recently been using the new Ilford *C*2 emulsions of thickness 40 μ. In some three square centimetres which have been examined, fifteen long tracks due probably to protons of energy 10–20 MeV., a track of length 180 μ which appears to be that of a slow meson coming to rest in the emulsion, together with a star of eleven charged particles have been found. Cloud chamber observations show that about one slow meson should be observed per eight slow protons[5], in agreement in order of magnitude with one per fifteen obtained with this plate.

A photograph of the meson track is shown in Fig. 1 *a*. It can be seen that the scattering along its length is appreciably greater than along an equal length of proton track shown in Fig. 1 *b*. This fact affords an approximate method of determining the mass of the meson[3]. The observed scattering is consistent with that of a particle having a mass of about 200 m_e, where m_e is the mass of an electron. A better value may be obtained from a second method which involves counting the number of developed grains per unit length of the track and comparing the results with similar measurements made along the proton track shown in Fig. 1 *b*, having a total length of 1040 μ in the emulsion. The number of grains n per 15 μ of track were counted, and a curve of n against range in the emulsion was plotted for each particle. The residual range of each particle was found from the curve for points having the same value of n (that is, when the two particles have the same velocity). This gives a value for the mass of the meson of 250 ± 50 m_e, but the actual value is inevitably subject to large statistical errors unless a large number of tracks are available for measurement. It is not possible to say whether or not the meson decayed with the emission of a β-particle, because an electron having energy equal to half the rest-mass energy of the meson (that is, ~ 50 MeV.) would leave no distinguishable track in the emulsion.

A single photograph of the star is shown in Fig. 2. The measured length of each track is given using the notation used by Powell and Occhialini[4], that is, (*e*) signifies that the particle stops in the emulsion, (*s*) that it leaves the surface, and (*g*) that it passes into the glass. *A*, *E*, *G* and *I* are probably due to protons of energy 10–20 MeV., while *C*, *F*, *H*, *J* and *K* may represent the tracks of α-particles of energy ~ 10 MeV. *B* appears to be the track of a particle which is just heavier than an α-particle; *D*, on the other hand, represents the track of a much heavier particle. Assuming that the star represents the disintegration of either a silver or bromine atom, these particles should be accompanied by about ten or twelve neutrons, and we estimate that the total energy given to the nucleus by the incoming particle

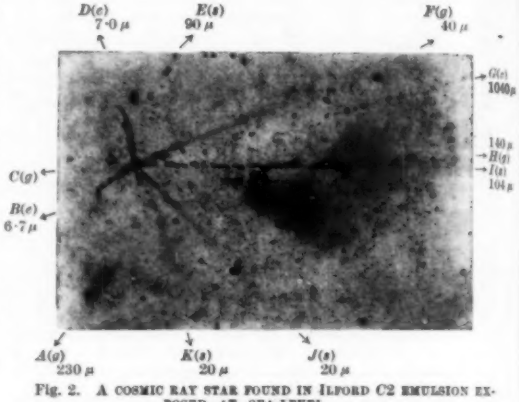

Fig. 2. A COSMIC RAY STAR FOUND IN ILFORD C2 EMULSION EXPOSED AT SEA-LEVEL

is of the order of 400 MeV. The proton and neutron intensities at sea-level are very weak, while fast negatively charged mesons make up about 35 per cent of the total radiation. Thus, this star may be another example of meson capture by a nucleus.

In conclusion, we wish to express our gratitude to Prof. R. M. Davies for many helpful discussions, to the heads of the Departments of Botany and Zoology for the loan of microscopes, and to Dr. C. F. Powell of the University of Bristol for bringing to our notice the new Ilford emulsions.

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April 8.

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A Variable Phase-Contrast System for Microscopy

PHASE-CONTRAST effects occur when the paths of the dioptric and diffracted rays between object and image differ sufficiently to produce some degree of interference. Were it possible to polarize the diffracted rays and dioptric rays at right angles to one another, a path difference could be introduced by passing them through a birefringent screen, the refractive index of which, being different for two perpendicularly polarized rays, would produce the

(a) (b) (c)

GLASS FRAGMENT IN GLYCOL PHTHALATE, MAGNIFICATION 25. (a) BRIGHT FIELD EFFECT WITH PHASE CONTRAST. (b) DARK FIELD EFFECT WITH PHASE CONTRAST. (c) APPEARANCE WITHOUT PHASE CONTRAST

effect of different optical thicknesses accordingly. Such a screen might be a mica plate causing a fixed difference of a quarter of a wave-length, or a compensator of the Babinet–Soleil type producing a variable difference.

Experiments have been carried out to test the possibilities of a variable phase-contrast system based on this principle. To reduce constructional difficulty in the initial stages, a simple arrangement was used, axial plane polarized light being employed with a 1-in. objective, at the back focus of which was mounted a mica retardation screen. This consisted of a quarter-wave plate having its central portion cut out, rotated 90°, and cemented back again. Consequently, the 'fast' direction of the centre and the 'slow' one of the rim were aligned, and while the dioptric rays from the specimen were focused in the central portion, the diffracted rays passed through the periphery. Thus the dioptric rays could be advanced by a quarter wave-length with respect to the diffracted rays, or, by turning the disk 90°, retarded by the same amount.

The accompanying photographs were taken through a retardation screen of 'Cellophane' built up in a similar way, but with an angle of about 45° between the 'fast' axes of the central part and the rim. By adjustment of the analyser, the dioptric ray can be weakened as desired, and by rotation of the disk relative to the polarization plane the appearance can be varied in the manner shown. The object, a fragment of glass ($n = 1{\cdot}54$) mounted in glycol phthalate ($n = 1{\cdot}53$), was chosen for its intractable nature, and the absence of double refraction checked.

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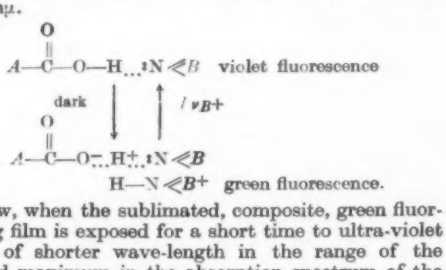
Proton Transfer between Organic Molecules Caused by Light

PROTON exchanges between acidic and basic organic compounds in solutions take place spontaneously according to the relative proton 'affinities' of the partners. The possibility of a proton transfer against the thermodynamic potential under the action of light seems not to have been demonstrated by a clear-cut experiment. In an investigation of photochemical reactions of organic compounds at low temperatures, we were able to show such an effect.

For the observation of a proton transfer without the intervention of a solvent, both components were sublimed *in vacuo* on to a cooled surface as a thin film in which a perfect mixture of the acidic and basic molecules was thus achieved. The experimental arrangement used was similar to that previously described[1]. As proton donors we chose easily sublimable crystalline organic acids, such as oxalic, benzoic, salicylic, succinic, terephthalic acids. As proton acceptor we used acridine, the well-known fluorescent indicator of *p*H in aqueous solutions, because the violet fluorescence of its neutral molecule is transformed into a bright green fluorescence in an acidified solution when this molecule incorporates a proton, being transformed into the acridinic cation with a corresponding change in the absorption and fluorescence spectra. This sharp change in the colour of the fluorescence was used here to detect the detachment of the proton from the acridinic cation under the action of light.

When both components condense from the vapour phase on to the surface kept at − 180° C., the close association of the acid with acridine will clearly lead to the formation of hydrogen bonds between the partners, as shown in the upper line of the diagram below, where the acid is designated by A, the basic molecule of acridine by B, their main groups being explicitly shown. At − 180° the violet fluorescence of the neutral acridine molecule rapidly changes, on standing, to a green fluorescence, this process being extremely rapid at room temperature. As can be judged from the position of the maximum in the fluorescence spectrum, this green fluorescence belongs to the acridinic cation, formed here at the cost of the proton detached from the acid (see lower part of the diagram). This process does not need any activation by light and occurs in a purely thermal way, equally well in the dark, as shown by the arrow pointing downwards. In all cases the fluorescence was excited by filtered ultra-violet radiation of the mercury lines at 366 mμ.



Now, when the sublimated, composite, green fluorescing film is exposed for a short time to ultra-violet light of shorter wave-length in the range of the second maximum in the absorption spectrum of the acridinic cation (about 250 mμ), the fluorescence of the exposed area assumes a violet colour with a spectrum characteristic of the acridine molecule (arrow pointing upwards). But if the short ultra-violet stimulation is cut off, this violet fluorescence gradually disappears, being replaced by the initial green one. The process can be repeated at − 180° many times without any indication of an irreversible change. We infer from this experiment that absorption of light by the acridinic cation has activated the N—H bond and effected the detachment of the proton with its simultaneous transfer to the acid anion in a kind of coupled predissociation (see dia-

gram). An energetic coupling between the partners must necessarily be assumed, since for succinic and terephthalic acids even relatively long wave-lengths in the first absorption maximum (at 360 mμ) of the acridinic cation are photo-active for this process. This reversible photo-reaction cannot be duplicated by the application of heat.

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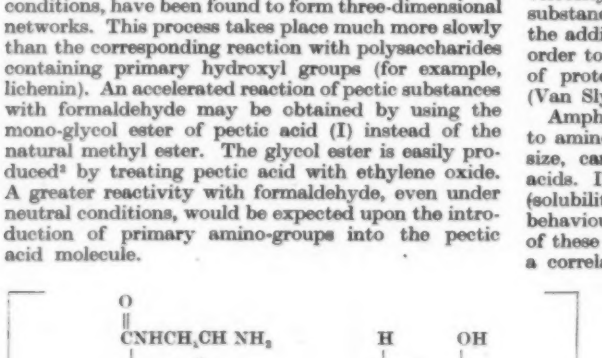
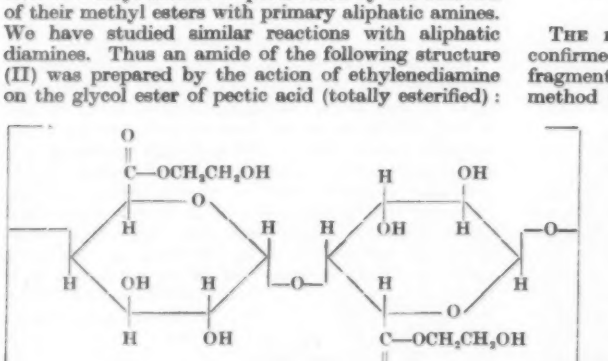
Derivatives of Pectic Acid as Models of Proteins

The constitution and properties of pectic substances have been intensively studied[1]. Pectins, when reacting with the bifunctional formaldehyde under acidic conditions, have been found to form three-dimensional networks. This process takes place much more slowly than the corresponding reaction with polysaccharides containing primary hydroxyl groups (for example, lichenin). An accelerated reaction of pectic substances with formaldehyde may be obtained by using the mono-glycol ester of pectic acid (I) instead of the natural methyl ester. The glycol ester is easily produced[2] by treating pectic acid with ethylene oxide. A greater reactivity with formaldehyde, even under neutral conditions, would be expected upon the introduction of primary amino-groups into the pectic acid molecule.



I

Carson[3] described a simple method for the preparation of *n*-alkyl-amides of pectic acid by the reaction of their methyl esters with primary aliphatic amines. We have studied similar reactions with aliphatic diamines. Thus an amide of the following structure (II) was prepared by the action of ethylenediamine on the glycol ester of pectic acid (totally esterified):



II

A complete conversion of ester to amide groups produces bases of high molecular weight, every link of the chain molecule carrying a branch with a primary amino-group at its end. The aqueous solution of this macromolecular substance is found to produce a gel easily when formaldehyde is added. These bases form insoluble salts with high molecular weight acids. Similar to the polyethylenimines[4], these ethylenediamine derivatives of pectic acid precipitate pectic acid itself.

If the conversion of ester to amide groups is incomplete, or if the amides formed undergo a partial hydrolysis, derivatives of pectic acid with a structure similar to that described above but containing a certain percentage of free carboxyl groups are obtained. In this case every chain molecule carries acidic carboxyl as well as basic amino-groups. These macromolecules possess a dipolar ionic structure, and in many respects show properties similar to those of proteins; for example, coagulation or minimum viscosity at the isoelectric point. Water-insoluble substances of this type can be rendered soluble by the addition of acid, alkali or even neutral salts. In order to characterize these ampholytes, the methods of protein chemistry have proved to be suitable (Van Slyke method, Sørensen titration, etc.).

Ampholytes with any desired proportion of carboxyl- to amino-groups, as well as ampholytes of different size, can be produced from esters of polyuronic acids. It should be possible to relate their properties (solubility, viscosity, electrochemical and optical behaviour, etc.) to the fairly well-established structure of these macromolecules, whereas with proteins such a correlation encounters, for the time being, much greater difficulties.

Further investigations are in progress.

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Detection of End Groups in Polymer Molecules

The radical theory of vinyl polymerization is confirmed by the identification of so-called catalyst fragments in the polymer molecule. The usual method is to purify the polymer by repeated reprecipitation and to use a catalyst containing an easily identifiable element. Thus when tribromobenzoyl peroxide has been used as the catalyst and the molecular weight of the product is not too high, it is possible to identify bromine in the polymer after repeated purification[1].

A method has now been found by which the presence of an end group in styrene polymerized with sodium sulphite[2] is readily demonstrated irrespective of the molecular weight and without any special purification of the polymer. The radical in

this case is monothionic acid, so that every polymer molecule is equivalent to an acid of very high molecular weight. When 1,000 ml. of a 0·2 per cent solution of such polymer in benzene is mixed in a gently rotating bottle with 10 gm. of solid caustic potash pellets, the viscosity of the solution gradually increases to a maximum of approximately twice its initial value over a period of perhaps 50 hours. With a 0·4 per cent solution the viscosity increases several times, while with a 0·05 per cent solution the viscosity is scarcely changed. By the addition of a standardized solution of hydrochloric acid gas in benzene, it is possible to titrate the potassium salt of the polymer by continuous determination of the viscosity. With the first small addition the viscosity remains unchanged, and with further addition it falls rapidly to a constant value which is unchanged even after the addition of a considerable excess of acid. The initial flat portion is thought to be due to traces of salts of acids of low dissociation constant which must first be neutralized before the salt of the polymer is attacked. The calculation of the number average molecular weight from end-group titration in this way, therefore, necessitates some knowledge of the dissociation constant. Assuming that the dissociation constant of polystyrene acid is equal to that of hydrochloric acid under the same conditions, then a polystyrene sample having a specific viscosity of 0·66 at a concentration of 0·1 per cent in benzene gave an equivalent weight of 32,000 by viscometric titration. For an exact interpretation the effect of surface adsorption in such dilute solutions would need consideration.

The alkali-treated polymer solution gives a specific viscosity/concentration curve which cannot be extrapolated to zero concentration with any certainty, whereas the polymer acid under the same conditions is more nearly linear, which gives further light on the Staudinger molecular weight rule.

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Misuse of 'Deduced Ratios' in the Estimation of Median Effective Doses

Dragstedt and Lang[1] wrote: "It seems to us obvious that a rabbit that died from 100 mgm. would have died of any higher dose and one that survived from 150 mgm. per kilogram would have survived from any lesser dose", and tabulated their "mortality percentages" as deduced according to "this rationale" from experienced ratios at the various doses. Behrens[2], with a similar idea, pointed out its limitations: "Unter der Voraussetzung, dass es sich um ein stetiges Kollektiv handelt, dass in den einzelnen Gruppen gleichviel Tiere angesetzt werden [or percentages in place of experienced effect ratios be used] und dass die Gruppenabstände nach beiden Seiten [of the expected median dose] gleichgross sind (arithmetische Reihe [elsewhere he correctly advocates a geometric progression]), ist zu erwarten, dass die Gesamptzahl der bei niederen Dosen Gestorbenen gleich ist derjenigen der Überlebenden bei höheren Dosen vom erwarteten Wert aus betrachtet. Selbstverständlich müssen wir die gesamte Ausdehnung unseres Kollektives mit den Gruppen erfassen." Behrens pointed out that the deduced ratio at a given dose is not the mortality ratio except at the value 1/2; that the ratios deal with areas on, rather than with ordinates of, the cumulative frequency curve.

Burn[3] briefly outlined the procedure and gave an example from Behrens, but did not mention the requirements for legitimate use, and, like Dragstedt and Lang, spoke of the deduced ratios as (deduced) mortality ordinates. Following such easy examples, many workers (including myself, though I was fortunate enough to discover the error before publication), have treated the ratios as mortalities at a given dose, with asymmetrical and/or non-geometric dosage schedule, unequal numbers of animals at dose-levels, and/or incomplete coverage of the mortality range (from high probability of no effects at one end to that of 100 per cent effects at the other). The consequences, apparent with some experimentation on log dose-probit charts, are more or less bias in the estimated median dose, and, in calling the ratios ordinates of the cumulative frequency curve, distortion from normality and reduction of *apparent* inter-animal dispersion. The bottom of the pitfall is thus attained when one attempts to estimate, in terms of the deduced ratios, the standard error of the so-deduced median dose[4]. The error is underestimated.

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The *Plesianthropus* Skull

Dr. R. Broom's account[1] of the new *Plesianthropus* skull makes it possible to take another step towards assigning to this form the place it occupies in the Primate series. His Fig. 2 shows clearly that the lacrimal and the ethmoid articulate over a wide area in the orbit. Possibly, the sphenoid and ethmoid articulate superficially in the anterior fossa (Fig. 1), although it is very difficult to accept Broom's hypothetical median part of the suture line as being correctly placed. In both cases the likeness with the Asiatic rather than with the African anthropoids is noted by Broom; but he makes no mention of the condition of the typically simian maxillary–premaxillary suture present on the face. The features that Broom has already described appear to verify the dictum of Klaatsch that "the less an ape has changed from its original form just so much the more human it appears", and perhaps agreement could be reached by assuming that *Plesianthropus* is a primitive and generalized anthropoid ape that has not attained to all the specializations characteristic of the modern apes, and is therefore nearer to the basal stock of the Primates from which man arose.

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ACQUIRED RESISTANCE TO PALUDRINE IN *PLASMODIUM GALLINACEUM*

Acquired Resistance and Persistence after Passage through the Mosquito

ACQUIRED drug-resistance is well known in trypanosomes and bacteria; but hitherto there have been only four accounts of this phenomenon in experimental work upon malaria. The Sergents[1,2], and Kritschewski and Rubinstein[3], claim to have induced resistance to quinine in *Plasmodium relictum*, though the Sergents' claim is based on one case only. Kritschewski and Halperin[4] state that the quinine-resistance sometimes, though not invariably, persisted after cyclical passage through *Culex pipiens*. Acquired resistance to plasmoquine and quinine was described by Nauck[5], and to plasmochin by Fulton and Yorke[6] in blood-inoculated strains of *P. knowlesi*.

Paludrine (No. 4888, N¹-*p*-chlorophenyl-N⁵-*iso*propylbiguanide), which was recently developed by Imperial Chemical (Pharmaceuticals), Ltd., is very active against avian[7] and against human malaria parasites[8,9], and promises to be a valuable drug in the treatment of malaria of man[10].

We have produced a forty-fold increase in resistance to paludrine in a strain of *Plasmodium gallinaceum* in chicks. In August 1946 we began to treat a normal strain of *P. gallinaceum*, passaged intravenously through chicks every second or third day, with the largest dose of paludrine hydrochloride (0·025 mgm.), which was without effect upon the parasites when given over a long period. Throughout this paper the doses are expressed in milligrams per 20 gm. of body weight of bird. After such treatment for a month, it was found that the parasites would tolerate a daily dose of 0·05 mgm. Thereafter the dose was gradually increased, as resistance to the drug developed, until after four and a half months parasites normal in appearance were developing in chicks receiving two doses daily each of 1·0 mgm., which is the largest dose that the host will tolerate.

The intensity of infections produced by standard inocula (50,000,000 parasites per chick) of the paludrine-resistant strain, in chicks receiving doses of paludrine ranging from 0·1 to 1·0 mgm. twice daily for three and a half days, was compared with similarly treated infections of the normal strain. Heavy infections were produced by the paludrine-resistant strain, whereas the development of the normal strain was inhibited completely.

The paludrine-resistant strain was tested for resistance to mepacrine hydrochloride and 4430 (N¹-*p*-chlorophenyl-N⁵-methyl-N⁵-*iso*propylbiguanide) acetate. In the minimum doses effective against the normal strain, the paludrine-resistant strain was not resistant to mepacrine, but showed definite resistance to 4430 (Table 1). Thus the resistance to paludrine is not specific but extends to the methyl homologue of the compound.

Attempts, extending over six months, to induce resistance to mepacrine in a normal strain of *P. gallinaceum* by the method used to produce paludrine-resistance failed; but after eight months treatment with pamaquin (= plasmochin) a normal strain of *P. gallinaceum* became resistant to the minimum effective dose (0·08 mgm.) of that drug.

TABLE 1. COMPARISON OF INTENSITY OF INFECTION OF PALUDRINE-RESISTANT AND NORMAL STRAINS

Normal strain	Paludrine-resistant strain	Normal strain	Paludrine-resistant strain
Untreated birds		Mepacrine, 0·64 mgm.	
1076 (3)	1030 (2)	11 (5)	3 (6)
Untreated birds		No. 4430, 0·4 mgm.	
800 (2)	843 (2)	0 (4)	640 (4)

The intensity of infection is expressed as the average number of parasites per 500 erythrocytes in groups of 2–5 birds. The number of birds in each experiment is given in brackets.

Exo-erythrocytic parasites similar in appearance to those produced in normal infections were formed by the paludrine-resistant strain.

Fairley and his co-workers[10] have shown that the development of *P. falciparum* and *P. vivax* is inhibited in the mosquito if paludrine, even in small quantities, is present in the infected blood. Such inhibition does not occur in the paludrine-resistant strain of *P. gallinaceum*. Thus four batches of *Aëdes ægypti* were allowed to gorge upon chicks infected with the strain of *P. gallinaceum* resistant to twice-daily doses of 0·4 mgm. of paludrine. Although the chicks were undergoing treatment with paludrine and had received a dose of drug four hours previous to being bitten, oocysts were numerous on the gut wall of the mosquitoes on the fourth day, and the salivary glands became heavily infected with sporozoites. These proved to be infective to chicks. The parasites which developed in the sporozoite-inoculated birds retained the resistance to paludrine (Table 2). The stability of the paludrine-resistance was further tested after three and five mosquito passages without intervening drug treatment. These passages covered a period of three months, and the results showed that there was no loss in paludrine-resistance (Table 2).

TABLE 2. COMPARISON OF INTENSITY OF INFECTION OF PALUDRINE-RESISTANT AND NORMAL STRAINS

Dose twice daily	Untreated birds: Normal strain	Untreated birds: Paludrine-resistant strain	Drugged birds: Normal strain	Drugged birds: Paludrine-resistant strain
	After 1 mosquito passage			
Paludrine				
0·4	1076 (3)	979 (2)	1 (5)	540 (5)
0·4	1001 (6)	903 (6)	0 (6)	466 (6)
0·4	1075 (3)	617 (3)	1 (6)	158 (5)
0·4	559 (4)	544 (5)	0 (4)	398 (5)
0·2				537 (6)
0·1*				720 (4)
	After 3 mosquito passages			
0·4	1240 (6)	685 (5)	0 (6)	268 (6)
0·2				451 (6)
	After 5 mosquito passages			
0·4	1344 (6)	862 (6)	1 (6)	398 (6)
0·2				784 (6)
0·1*				945 (6)

* Minimum dose effective on normal strain.
The intensity of infection is expressed as the average number of parasites per 500 erythrocytes in groups of 2–6 birds. The number of birds in each experiment is given in brackets.

Apart from Kritschewski and Halperin's observations, experiments upon acquired drug resistance have hitherto been made upon organisms in which a sexual cycle of development is not known. The persistence of paludrine-resistance in *P. gallinaceum* after transmission by the mosquito is therefore of particular interest, as a definite sexual cycle of development is involved. Whatever may be the mechanism which produces this resistance, whether it be the selection of favourable mutants or the

gradual adaptation of the parasites to the drug, resistance so produced is maintained through at least five sexual cycles, and also persists after a long sojourn in an environment (the mosquitoes' tissues) entirely different from that in which it was evolved.

A detailed account of these results will be published later. We are indebted to Imperial Chemical (Pharmaceuticals), Ltd., for the gift of the paludrine, 4430, pamaquin and mepacrine. The work has been financed by the Medical Research Council.

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Effects of Paludrine and Other Antimalarials

THE facility with which pathogenic protozoa and bacteria may acquire resistance to therapeutic agents, as a result of non-curative treatment of infections by these pathogens, is of profound significance both for the theory and for the practice of chemotherapy. The readiness with which trypanosome species may thus develop resistance to certain types of compound is well known; but malaria parasites in general have hitherto appeared singularly incapable of developing an acquired resistance, although a number of claims to have demonstrated this property have been made. Only one of these claims, that of Fulton and Yorke[1], referred to again below, can be incontrovertibly accepted.

Lourie[2] treated infections of *P. cathemerium*, in serial passage through canaries, with an intraperitoneal injection of 1 mgm. quinine hydrochloride every day for seventeen months, without producing quinine-resistance; similar treatment with 0·01 mgm. pamaquin (plasmochin) every day for nine and a half months failed to give rise to pamaquin-resistance. Fulton[3], working with *P. gallinaceum* in chickens, also failed to produce pamaquin-resistance by means of intermittent short courses of intensive treatment over a period of fifteen and a half months, and he suggested that the explanation may lie in the fact that the exo-erythrocytic forms, discovered by James and Tate[4], do not respond to treatment and therefore remain as a source of further pamaquin-sensitive erythrocytic forms. Fulton and Yorke[1] were able to produce pamaquin-resistance in *P. knowlesi* (in which exo-erythrocytic stages have not yet been demonstrated) in monkeys.

We have failed to produce resistance to quinine hydrochloride, mepacrine methanesulphonate, sulphadiazine, or 3349 (a forerunner of paludrine, see Curd, Davey and Rose[5]) in *P. gallinaceum*, in the course of intensive treatment of serially infected chicks, for periods extending over more than two years, as shown in the accompanying table (series 1 to 4). At the end of these periods the drug-treated strains proved to be as sensitive to the respective compounds as they were at the start.

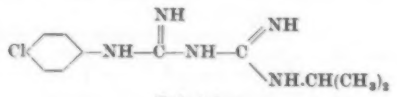
In the case of paludrine, however, a high degree of resistance appeared within about three months, in serially subinoculated and treated infections, in each of two experiments (series 5 and 6). In series 5 the infections were treated with doses of the order of the minimum effective for the normal strain, and in series 6 with doses in the region of the maximum tolerated by young chicks. The first indication of resistance in series 5 appeared two and a half months from the start (after a total of 54 doses of 0·25 mgm. per 50 gm. body-weight), and a month later, that is, at the end of the three and a half month period quoted in the table, infections were completely resistant to a course of seven doses of 0·25 mgm. in three and a half days. In series 6 a high degree of resistance—at least a tenfold increase—appeared suddenly at the end of the three-month period quoted in the table, a period which includes one month during which no treatment was administered. Both strains were then subjected to more treatment, in further serially inoculated chicks (a further aggregate of 3·5 mgm. in eleven days for series 5, and 70 mgm. in one month for series 6). Infections with parasites of each of these strains were then compared with parent strain infections, in respect of the 'minimum effective dose', as defined and as determined by the technique of Davey[6]. By this measure, the parasites of series 5 appeared to be about twenty times, and those of series 6 at least twenty to forty times, as resistant to paludrine as parasites of the parent strain.

HISTORY OF DRUG TREATMENTS IN SIX SERIES OF *P. gallinaceum* INFECTIONS

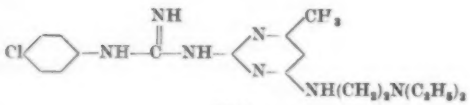
Series	Treated with	Duration of treatment: Total (months)	Duration of treatment: No. of successive passages	Dosage (by stomach tube): Size of each dose (mgm. per 50 gm.)	Dosage (by stomach tube): Total no. of doses	Dosage (by stomach tube): Aggregate dosage for series (mgm. per 50 gm.)	Result
1	Quinine	29	73	7·5	516	3,870	No acquired resistance
2	Mepacrine	27	69	10·0	438	4,380	
3	Sulphadiazine	27	44	25·0	500	12,500	
4	3349	28	73	10·0	483	4,830	
5	Paludrine	3½	16	0·25	75	19	High degree of acquired resistance
6	"	3	11	5·0 (occasionally 2·5 or 0·25)	32	119	

The paludrine-resistant strain of series 6 was found to be normally sensitive to quinine, mepacrine, sulphadiazine, and 3349. The absence of acquired resistance to 3349, on the part of the paludrine-resistant strain, coupled with the failure to produce resistance by prolonged exposure to 3349, is of special interest in view of the similarity between paludrine and 3349 in respect of prototropic possibilities, stressed by Curd, Davey and Rose[7] as associated with antimalarial activity in compounds of this general type.

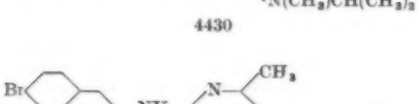
The paludrine-resistant strain was found to be fully resistant also to 4430 [7], a close analogue of paludrine, and both these compounds lack the pyrimidine ring of the earlier prototypes of Curd, Davey and Rose which included 3349. This suggests that it may be the pyrimidine ring which determines the widely divergent behaviour of 3349 and paludrine in respect of acquired drug resistance, an interpretation which is further supported by the fact that the paludrine-resistant strain has been found to be normally sensitive also to 3502 [5], a pyrimidine analogue of 3349.



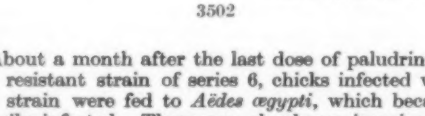
Paludrine



3349



4430



3502

About a month after the last dose of paludrine to the resistant strain of series 6, chicks infected with the strain were fed to *Aëdes ægypti*, which became heavily infected. There was clearly no impairment of transmissibility of this strain through mosquitoes, and when tested after mosquito-passage the strain was found to be as resistant to paludrine as before.

We are obliged to Dr. J. D. Fulton and Dr. J. W. S. Macfie for their co-operation in the early stages of this work, which was aided by a grant from the Medical Research Council. We are also grateful to Imperial Chemical Industries, Ltd., for supplying some of the compounds used. Full details will be published in the *Annals of Tropical Medicine and Parasitology*.

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PHYSIOLOGY OF PATTERN FORMATION IN MALE AND FEMALE FEATHERS OF BARRED PLYMOUTH ROCK FOWLS

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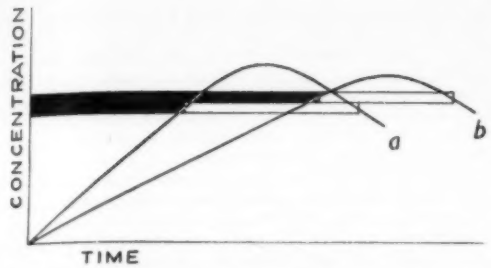
ONLY very recently was I able to read Nickerson's paper[1] on barred pattern formation in chicken feathers. Nickerson's experimental analysis by means of melanophore transplantation according to H. B. Willier's technique, with other experiments and observations, demonstrates that the barring rhythm is determined by the melanophores themselves rather than by their reaction to some extrinsic rhythm or pre-existing pattern in the epidermal substratum of the feather germ (the latter view was maintained by Henke[2]). Thus barring is determined not only by the action of the genes independently in each follicle, as I supposed[3], but also by a rhythm inherent in the melanophores of each follicle. Furthermore, the suggestive hypothesis is put forward that "the barring rhythm is controlled by the action of melanophores through the medium of some diffusible substance which is capable of inhibiting the formation of pigment by melanophores present in the subjacent white regions of the developing feather germ"[1]. Such an interpretation fits well, as Nickerson points out, with the correlation that I have demonstrated between pattern characters and rate of growth of the feather, including the negative correlation between width of the white bar and growth-rate.

I should like now to direct attention to another point which has not been discussed by Nickerson, namely, the sexual dimorphism of pattern. The structural differences in feathers of certain tracts (for example, neck, saddle) between cock and hen are dependent on sex hormones[4] just as in many other breeds of fowl; but even in structurally identical feathers (for example, flights, breast feathers, etc.) a sexual dimorphism exists which has been analysed by Agostini[5]. The width of double bar (black plus white) is smaller in the male than in the female, both in juvenile and adult plumage (average ratio on breast feathers *c.* 0·78–1). The width of the black bar with respect to white is also smaller in the male feather: in the adult plumage (breast) of the cock the black bar comprises, on an average, 50 per cent of the double bar, while in the hen it comprises as much as 70 per cent. In other words, the male has a smaller double bar and a larger white bar than the female. These differences are statistically valid[5]. They do not depend on sex hormones, because castration, ovariectomy[5] and transplantation[3,6] do not alter them. Therefore, they are most probably directly determined by genetical constitution. Since barring is a sex-linked character and since the male is heterozygous in birds, one can suppose that such a phenotypic effect might be due to the presence of two doses of the *B* gene in the male and only one in the female[5]. How does this interpretation fit in with Nickerson's hypothesis?

Before testing this point, it must be emphasized that the different width of bars and different white/black ratios cannot be determined by different growth-rates in homologous feathers of male and female. I did not find any substantial difference in rate of



AVERAGE WIDTH OF THE BLACK AND WHITE BARS IN THE FEMALE (*above*) AND MALE (*below*) ANTERIOR BREAST FEATHERS OF BARRED PLYMOUTH ROCKS. HYPOTHETICAL CURVES OF THE CONCENTRATION OF THE PIGMENT-INHIBITING SUBSTANCE IN THE MALE (*a*) AND FEMALE (*b*) FEATHER GERMS AND THEIR THRESHOLD VALUES ARE ADJUSTED TO FIT THE ACTUAL WIDTH OF BARS

growth between homologous flights of a cock and a hen with very marked sexual dimorphism of the pattern[3]. Fluctuations in growth-rates of feathers of the same tract certainly do occur in different individuals, and possibly a small consistent sexual difference also exists. Unfortunately, I have not been able, for a long time, to work with chickens and therefore I have not been able to collect sufficient data relating to this point. In any event, from published and some unpublished data I also find that the differences in growth-rate between male and female breast feathers are not sufficiently high to account for the differences of pattern. In fact, one can calculate, starting from the male pattern, the growth-rate expected in order to account for the width of the female double bar. Pooling together the figures for three cocks and two hens, the average growth-rate of the hen's anterior breast feather should be, in order to agree with the width of pattern, $2{\cdot}022 \pm 0{\cdot}0265$ mm. per day, while it is actually $1{\cdot}865 \pm 0{\cdot}0243$; in the hen's posterior breast feather it should be $2{\cdot}340 \pm 0{\cdot}0258$, while it is in fact $2{\cdot}160 \pm 0{\cdot}0381$. The differences between the expected and observed figures are: $0{\cdot}157 \pm 0{\cdot}0359$ and $0{\cdot}180 \pm 0{\cdot}0460$, that is, they are significant. Therefore, I think that a difference in rate of growth, even if statistically ascertained, could not explain the different width of the double bar, to say nothing of the fact that the higher the rate of growth, the greater should be the proportion of white to black[3], which is just the contrary of what occurs.

The larger proportion of white in the male feather is easy to understand on the above assumption. As has often been pointed out, the genetical and physiological evidence is quite convincing that the *B* factor acts as a rhythmic suppressor of pigmentation: the positive effect, in barring, is inhibition of pigmentation, and Nickerson's hypothesis is in line with this view. Thus a higher dose of *B* gene (*BB* genotype in male) would presumably form a greater amount of the inhibiting substance, which would interrupt pigment formation for a longer time (about 1·3 to 1 is the male to female white bar ratio, absolute width, in the breast feather) than the *BO* female genotype.

It is more difficult to see why the width of the double bar should be larger in the hemizygous sex. Supposing that a threshold of a concentration of the inhibiting substance must be reached for the melanophores to react, and that the rate of formation of the substance is in some way proportional to the dose of *B* gene present; in the *BO* genotype the melanophores may work for a longer time (about 1·84 to 1 is the female to male black ratio, according to Agostini's average data) in forming pigment, before the inhibiting substance reaches the threshold concentration (hence the greater width of black bars). Once it is reached, the elimination of the substance should take place more rapidly (because of its lower total concentration), hence the absolute narrowness of the white bar. Since the black bar alone is, on the average, 1·84 times wider in male than in female breast feather, even a relatively short duration of the 'white' phase will bring about a greater width of the total bar in the female. The process might be represented in a diagrammatic way as in the accompanying illustration, where the relative width of black and white bars in male and female feathers are taken from the mean values of Agostini's data, while the curves of concentration of the inhibiting substance are, of course, entirely hypothetical and roughly adjusted to fit the actual morphological data.

[1] Nickerson, M., *J. Exp. Zool.*, **95**, 361 (1944).
[2] Henke, K., *Zool. Anz. Suppl.*, **8**, 176 (1935); *Biol. Zentrbl.*, **59**, 459 (1939).
[3] Montalenti, G., *J. Exper. Zool.*, **69**, 269 (1934).
[4] Montalenti, G., *Atti V Congr. Mond. Pollicolt., Roma*, 1933, **2**, 397 (1934); *Rass. Clin. Terap. e Sci. affini*, **23**, 1 (1934).
[5] Agostini, A., *Arch. Zool. Ital.*, **26**, 319 (1939). Montalenti, G., Proc. VII Int. Genet. Congr., 1939, 205 (1941).
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BIOLOGICAL INVESTIGATIONS IN THE AEGEAN SEA

By PROF. ENRICO TORTONESE
Zoological Institute and Museum, University of Turin

AS is well known, the eastern Mediterranean has been far less biologically explored than the western. It was therefore a very welcome occasion to spend several months (1943) in the Island of Rhodes (Aegean Sea) and carry out there a series of researches, the main result of which has been to add a number of species to those previously found and recorded. Knowledge of Aegean animals was first improved by E. Forbes about a century ago. In 1926 a group of Italian zoologists investigated the shores of Rhodes and neighbouring islands; other scattered data are to be found in various scientific papers, such as those concerning the famous *Thor* expedition. New and careful investigations are desirable for both an exact understanding of geographical distribution, variations and migrations of Mediterranean species and for ascertaining the spreading of those which have immigrated from the Red Sea through the Suez Canal: it is certain that some of them have reached the Aegean Sea, following the main current along the coasts of Syria and Asia Minor.

Of such zoological groups as Porifera, Cnidaria, Echinodermata, Nemertea, Tunicata and fishes no collections had hitherto been made in Rhodes and nothing is actually known. The marine fauna does not seem to be a very rich one; both species and individuals appear relatively scarce and it is improbable that this may be due to lack of proper researches. Steepness of submarine soil and frequent roughness of sea, consequent on almost continuous and violent winds, are probably responsible for this scantiness

of life. General physiognomy, however typically Mediterranean, differs somewhat from that of the western shores of this Sea, as well as from that of its more northern parts; a number of species, for example, fishes, are peculiar to warm waters. Field work (hindered by war conditions) on various types of sea-bottom and sea-shore has revealed some noteworthy biocænosis. The predominance of *Cardita sulcata*, a lamellibranchiate molluscan, along the sandy beaches was already known; a peculiar association of *Asterina* and *Chiton* under the stones was now noted on rocky bottom in the harbour, with plenty of seaweeds; various other invertebrates were sharing the same habitat. Calcareous sponges, sea-anemones (*Anemonia sulcata*), two species of Nemerteans, free and sedentary Polychæta, Bryozoa (among them *Zoobotryon pellucidum*) and Ascidians were found in the harbour of Rhodes city and its neighbourhoods. *Actinia equina* seems to be missing, but on the opposite shores of Anatolia a good number of fine specimens were found. In shallow water during summer *Aplysia* was plentiful and grazing among seaweeds; a good series of other Mollusca were collected, too. Among other species were *Tethys leporina* and two different *Octopus*. Colonies of *Pyrosoma* and some Hydromedusæ were the only pelagic animals that could be obtained. Twenty species of Echinoderms were gathered: among them *Tethyaster*, *Ceramaster* and *Centrostephanus* were quite common in rather deep water, more or less distant from the shore. Crustaceans were fairly abundant, and among the largest Malacostraca *Squilla mantis*, *Scyllarides latus*, *Maia squinado*, *Calappa granulata* and *Dromia vulgaris* do not appear to have been previously noticed, as well as *Pagurus striatus*, a common dweller of large shells of *Cassis sulcosa* and *Murex trunculus*.

Fishes were particularly looked for, and collected species amounted to 116, of which six were Plagiostomia. Fishermen are few, and their work even in normal times is not a profitable one. The cause is to be found both in scarcity of plankton, that does not foster a rich fish-life, and in the nature of the sea-bottom, that forbids using some fishing technique otherwise advantageous. Ambiental conditions are unfavourable to fishes the life of which is largely dependent on development of continental shelf, as are Heterosomata, of which four species only were obtained. Shoals of migratory fishes (Scombroids, etc.) coming from the Black Sea or going there appear near Rhodes, but cannot support fisheries on an industrial scale. As a whole, these animals are much more numerous along the coasts of Asia Minor. Large sea-basses, mullets, red mullets, hakes and some sparoids were most frequently observed to be sold. This fish fauna shows a rather tropical character, that is stressed not only by such chiefly tropical families as Labroids and Carangoids or genera such as *Sardinella* and *Sparisoma*, but also by the presence of Red Sea immigrants. Only one of these, *Siganus rivulatus*, had previously been found in Aegean waters. Two other species may now be added—*Hemirhamphus marginatus* and *Monacanthus setifer*, the latter hitherto found only in Palestine. Apart from some species such as the small ray (*Raia radula*), that are more or less localized and the recognition of which is therefore interesting, four species, of which a single specimen was found in Rhodes, deserve particular mention. *Lobotes surinamensis*, a percoid, and *Remilegia australis*, a remora (sucking-fish), are tropical species of which we had but one Mediterranean record: the former was captured in Palermo (1875), the latter in Trieste (1881). *Scyris alexandrinus*, a form of deep-bodied carangoid, seems to be rare and until now was reported only from North Africa and Syria. *Gobius thori* is a little goby of which only the type specimen, collected by the *Thor* near Tenedo (northern Aegean Sea) many years ago, was known.

All this material was unfortunately lost, but all notes and drawings were fortunately preserved, so that it has been possible to deal with it particularly in separate papers, that will appear—it is hoped—as soon as printing difficulties will allow.

RADIOCHEMISTRY IN BRITAIN

OPENING OF THE LONDONDERRY LABORATORY AT DURHAM

GREAT BRITAIN was in the forefront of original research on the chemistry of the radio-elements so long as Frederick Soddy, after his return from Montreal, was devoting his activities to this field. Much of the experimental material which led to his conception of isotopy was collected in his laboratories in Glasgow by A. Fleck; the first thorium-lead was prepared there and its atomic weight determined; and protoactinium was discovered by Soddy and Cranston in Aberdeen simultaneously with Hahn and Meitner in Berlin-Dahlem. No chemical laboratory in Great Britain, however, has followed up the tradition, so that from about 1918 on, the centres of radiochemical research were the Laboratoire Curie in Paris, and the Radium Department of the Kaiser Wilhelm Institut für Chemie at Dahlem, under Otto Hahn. How important their work was may be seen from the one fact that the discovery of the fission of uranium was an achievement to which the systematic study of the chemical products of the uranium irradiation was bound to lead, and to which the Paris laboratory had already come very close when Hahn and Strassmann's fundamental paper appeared. In Britain no corresponding work was undertaken, in spite of several physicists being greatly interested in the problem.

The need for a revival of radiochemistry was keenly felt during the Second World War, when the military and economic possibilities of the use of atomic energy had been recognized, and in Montreal a joint British–Canadian Government team, first under the directorship of Dr. H. Halban, and later under that of Prof. J. D. Cockcroft, did research work on uranium fission. The chemistry division of that team, built up during the years 1943–45 under Prof. F. A. Paneth, comprised finally more than thirty graduates, many of whom had been recruited in Britain.

It would have been deplorable if at the end of the War still no British university had shown interest in the study of radiochemistry. Fortunately, the University of Durham, with the support of the University Grants Committee, found ways and means of building a laboratory in which radiochemical specialists can carry out their researches and, at the same time, train young chemists in the particular techniques which will enable them to collaborate with the departments of nuclear physics in universities and Government establishments, and also with

biologists interested in the use of radioactive tracers. The laboratory, designed by Dr. E. Glueckauf and Mr. L. Gradon, consists of two modest one-story buildings near the main Science Laboratories, one equipped for the chemical treatment of radioactive substances of low activities, the other for those emitting strong radiations.

This Laboratory—named after Lord Londonderry, chancellor of the University of Durham, who has personally contributed to the cost of its erection by a generous gift—was formally opened on June 4 by Viscount Portal of Hungerford, on whom the honorary degree of D.C.L. was conferred on this occasion. The ceremony was attended by Sir Wallace Akers, who during the War was director of the United Kingdom Government's atomic energy project; Dr. A. Fleck, Soddy's old collaborator; Dr. J. D. Cockcroft, now in charge of the Atomic Energy Research Establishment at Harwell; Dr. J. Guéron, of the Commissariat de l'Energie Atomique in Paris; and many other well-known men of science. Lord Portal, who after his brilliant R.A.F. career is now directing the Government's efforts towards the peace-time utilization of atomic energy, replied with a speech which dealt largely with the relationship of British universities to the State. We print below some extracts from Lord Portal's address.

The practice of the universities of Great Britain in honouring members of the fighting services with degrees is, in my humble opinion, a very sound one, quite apart from the personal pleasure it gives to the recipients. It tends to emphasize one of the clearest lessons of the late War, that to secure the greatest possible efficiency the fighting services must call to their aid, in peace as well as in war, all the best talent in civil life, and make full use in many different ways of the type of brain-power that is to be found in the universities rather than in the fighting services in peace-time. The closer you can draw the bonds between the universities and the fighting services, the better for the country.

Much of the special scientific work done in the War by, or under the guidance of, members of the staff of the University of Durham was of great importance, directly or indirectly, to the fighting services. Under such distinguished men as Profs. Curtis, Riley and Wager, Drs. Valentine, Richardson and Varley, and Mr. Cragg, problems ranging from camouflage to the case-hardening of steel; from air photography to agricultural pests; from hydrodynamics to the dehydration of food, were tackled with great success.

But to-day we are thinking mainly of radiochemistry, because this new Laboratory will be the first school in that subject to be formed in Great Britain, and the subject is one of very great importance if the peaceful uses of atomic energy are to be fully developed. It is good news that the school is to be under the charge of so eminent a man as Prof. F. A. Paneth, whose reputation extends far beyond this country and whom we were all delighted to see honoured recently with the fellowship of the Royal Society. He and some of his associates from this University, Drs. Chackett and Musgrave, and Mr. Martin and Mr. Morgan, did valuable work in Canada during the War, and there is much for them to do here in the future. For without skilled radiochemists in adequate numbers, we shall not be able to develop fully the use of the radioactive isotopes which we hope will be of such enormous benefit to future research in medicine, agriculture and industry generally.

I hope you will not think it inappropriate if I conclude with one or two observations which are not strictly relevant to this occasion or to this University.

During the last year my work has brought me into close contact with many of the most eminent men of science in Great Britain. Perhaps it is a bit late for me to hope to make good the omissions of a classical education, but I can say with truth how enormously I respect these great men and the power and integrity of their brains, and how immensely helpful I have found their advice, even when I am unable to follow their calculations. In the course of my contact with them, and in reading what others have written, I have realized the depth of feeling which exists far and wide in the world of science about the responsibility of scientific men for the consequences of the discoveries they have made in the realm of nuclear fission.

Like the discovery of flying, this atom business is in many ways very awkward. I sympathize very much with the scientific men. It is perfectly true that the prospects for the good uses of atomic energy which are already foreseen, and for the discovery of others not yet foreseen, are at present gravely affected by international distrust and the resulting need for secrecy and strict Government control of the whole business. Secrecy is in itself a brake on progress, and if it has to be continued indefinitely it may well drive some good men in disgust to other lines of research where freedom is allowed and can do no harm. Government control, though it is absolutely necessary under present conditions, and perhaps always will be, means that the best brains in industry, particularly on the engineering and metallurgical sides which are every bit as important as the nuclear-physical side, are difficult to mobilize upon the problems of development.

No one could recognize all this more clearly than we officials who have to help to administer the law, but it really is no use pretending that conditions to-day are appropriate for the casting of all secrecy to the winds in the name of the freedom of science. What we can and will do is to try, consistently with our duty, to minimize the inconvenience caused by the restrictions and to interpret the will of Parliament in a liberal spirit.

I do not think that, on the long view, there need be any uneasiness about the British contribution to atomic energy, though the rate of progress is bound to be affected by the number of urgent things Britain has to tackle simultaneously at the present time. We may not have the volume of human and material resources that some other nations have, and the maintenance of a proper balance between research directed to the execution of a short-term programme and that required for the needs of long-term progress and training will always be a problem. But I do believe we have in Britain the quality of human genius which will before long put us in the very front rank. Quality is what we must bank on, and it is to the British universities that we must chiefly look to foster and maintain the quality of future generations of our scientists and engineers.

May I express the hope and belief of all who are here to-day that the Londonderry Laboratory will see long and useful service, and that the work done there will be outstanding in its contribution to the ultimate benefit of mankind.

FORTHCOMING EVENTS

Tuesday, July 1

ROYAL SOCIETY OF MEDICINE (at 1 Wimpole Street, London, W.1), at 1.30 p.m.—Annual General Meeting.

Wednesday, July 2—Thursday, July 3

IRISH CHEMICAL ASSOCIATION (joint meeting with the DUBLIN SECTION of the ROYAL INSTITUTE OF CHEMISTRY, in the Department of Chemistry, University College, Upper Merrion Street, Dublin).—Colloquium on "The Industrial Utilization of Agricultural Products and of Seaweed".

Wednesday, July 2

At 10.30 a.m.—Prof. J. L. Simonsen, F.R.S.: "Agricultural Products and the Chemical Industry"; at 2.30 p.m.—Prof. J. B. Speakman: "The Production, Properties and Uses of Seaweed Rayon".

Thursday, July 3

At 10 a.m.—Prof. E. L. Hirst, F.R.S.: "The Utilization of Carbohydrate Products".

Friday, July 4

GEOLOGISTS' ASSOCIATION (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 6 p.m.—Dr. K. P. Oakley: "Geological Safari in East Africa".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

RESPONSIBLE LECTURER IN BIOLOGY, and a RESPONSIBLE ART MASTER, in the Municipal Technical College and School of Art—The Director of Education, Education Offices, Library Street, Blackburn (July 5).

READERSHIP IN MINING tenable at King's College, Newcastle-upon-Tyne—The Registrar, University Office, 45 North Bailey, Durham (July 5).

HEAD OF THE DEPARTMENT OF HORTICULTURE—The Principal, Yorkshire Institute of Agriculture, Askham Bryan, York (July 5).

LECTURER IN ENGINEERING WORKSHOP SUBJECTS, with special qualifications in Automobile Engineering—The Principal, Hendon Technical College, The Burroughs, Hendon, London, N.W.4 (July 5).

SENIOR ASSISTANT IN MECHANICAL ENGINEERING—The Principal, Harris Institute, Corporation Street, Preston (July 5).

HEAD OF THE BUILDING AND CIVIL ENGINEERING DEPARTMENT—The Principal, Royal Technical College, Peel Park, Salford 5 (July 5).

LECTURER IN THE ENGINEERING DEPARTMENT of the County Technical College, Wednesbury—The Director of Education, County Education Offices, Stafford (July 5).

ORGANIC CHEMIST at Long Ashton Research Station—The Secretary and Registrar, The University, Bristol 8 (July 7).

LECTURER IN MECHANICAL ENGINEERING—The Principal, Dudley and Staffordshire Technical College, Dudley (July 8).

HEAD OF THE ELECTRICAL ENGINEERING DEPARTMENT of the Rutherford College of Technology—The Director of Education, Education Offices, Newcastle-upon-Tyne (July 8).

LECTURER IN CHEMISTRY, a LECTURER IN PHYSICS, and a LECTURER IN MECHANICAL ENGINEERING—The Principal, West Ham Municipal College, Romford Road, Stratford, London, E.15 (July 12).

ASSISTANT LECTURERS (2) IN PHYSICS—The Registrar, The University, Manchester 13 (July 12).

PROFESSOR OF PHYSICS at the Natal University College, Pietermaritzburg—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (July 15).

ASSISTANT LECTURER IN FUEL TECHNOLOGY in the Department of Metallurgy—The Registrar, University College of South Wales, Cathays Park, Cardiff (July 15).

CHIEF CHEMIST (grade of Principal Scientific Officer or Senior Principal Scientific Officer), SCIENTIFIC OFFICERS or SENIOR SCIENTIFIC OFFICERS (Chemists and Physicists), and TECHNICAL OFFICERS in the Testing and Dyeing and Finishing Departments—The Secretary, Wool Industries Research Association, Torridon, Leeds 8 (July 19).

SPEECH THERAPIST—The Clerk to the Governors, St. Bartholomew's Hospital, London, E.C.1 (July 19).

CHAIR OF PHYSIOLOGY at Charing Cross Hospital Medical School—The Academic Registrar, University of London, Senate House, London, W.C.1 (July 21).

ASSISTANT PROFESSOR OF APPLIED CHEMISTRY (Organic), an ASSISTANT PROFESSOR OF APPLIED CHEMISTRY (Inorganic), and LECTURERS (4) IN ORGANIC or INORGANIC or APPLIED CHEMISTRY or BIOCHEMISTRY, at the Industrial Chemistry Institute, Alexandria—The Director, Egyptian Education Bureau, 4 Chesterfield Gardens, London, W.1 (July 31).

PROFESSOR OF THEORETICAL PHYSICS, an ASSISTANT PROFESSOR OF PHYSICS, an ASSISTANT PROFESSOR OF APPLIED MATHEMATICS, a LECTURER A OF APPLIED MATHEMATICS, and a LECTURER A OF EXPERIMENTAL PHYSICS, in the Faculty of Science, Farouk I University, Alexandria—The Director, Egyptian Education Bureau, 4 Chesterfield Gardens, London, W.1 (July 31).

RESEARCH FELLOWSHIP (Organic Chemist)—The Secretary, Institute of Child Health, Hospital for Sick Children, Great Ormond Street, London, W.C.1 (July 31).

SENIOR LECTURER IN MATHEMATICS in the University of Melbourne—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (July 31).

LECTURER IN PHONETICS—The Secretary, The University, Edinburgh (July 31).

LECTURER IN CHEMISTRY in the University of Adelaide—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (August 1).

ASSISTANT LECTURER IN THE DEPARTMENT OF EMBRYOLOGY—The Secretary, University College, Gower Street, London, W.C.1 (August 1).

JUNIOR LECTURER IN PHYSICS in Canterbury University College, Christchurch, New Zealand—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (August 8).

LECTURER IN ANTHROPOLOGY in the University of Cambridge—Dr. G. E. Daniel, St. John's College, Cambridge (September 20).

BOTANIST to the Empire Potato Collection, Cambridge—The Secretary, Imperial Agricultural Bureaux, 2 Queen Anne's Gate Buildings, London, S.W.1 (September 30).

DIRECTOR OF THE PARASITOLOGICAL LABORATORY, Baghdad—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.N.13725.

HEAD OF THE MATHEMATICS DEPARTMENT—The Principal, Huddersfield Technical College, Huddersfield.

LECTURER IN PHYSIOLOGY AND ZOOLOGY—The Registrar, Technical College, Sunderland.

LECTURER IN CHEMISTRY (Grade I) in the School of Bakery—The Secretary, Royal Technical College, Glasgow.

LECTURER IN FUEL TECHNOLOGY, and a DEMONSTRATOR (temporary) in the DEPARTMENT OF PHYSICS—The Registrar, King's College, Newcastle-upon-Tyne.

LECTURERS IN (*a*) CHEMISTRY to B.Sc. standard, (*b*) BIOLOGY to Inter.B.Sc. standard, and (*c*) MATHEMATICS, PURE AND APPLIED, to B.Sc. standard—The Clerk, Northern Polytechnic, Holloway Road, London, N.7.

LECTURERS (2) IN MATHEMATICS—The Principal and Clerk to the Governing Body, Wigan and District Mining and Technical College, Wigan.

HEAD OF THE DEPARTMENT OF CHEMISTRY—The Clerk, Northern Polytechnic, Holloway Road, London, N.7, endorsed "H".

ASSISTANT LECTURER AND DEMONSTRATOR IN THE PHYSICS DEPARTMENT—The Secretary, King's College of Household and Social Science, Campden Hill Road, London, W.8.

LECTURER IN AGRICULTURAL CHEMISTRY (Animal Nutrition) in the Faculty of Agriculture and Horticulture—The Registrar, The University, Reading.

LECTURER, and ASSISTANT LECTURERS (2), IN THE DEPARTMENT OF BIOCHEMISTRY—The Secretary, University College, Gower Street, London, W.C.1.

TECHNICIAN (Grade A), and TECHNICIANS (2, Grade B), with special knowledge of hæmatology—The Medical Superintendent, Dudley Road Hospital, Birmingham 18.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Abstracts of World Surgery, Obstetrics and Gynæcology. Vol. 1, No. 1, January. Pp. ii + 70 + viii. (London: British Medical Association, 1947.) 4*s.*; 42*s.* per annum. [221

Abstracts of World Medicine. Vol. 1, No. 1, January. Pp. ii + 110 + xii. (London: British Medical Association, 1947.) 6*s.*; 63*s.* per annum. [221

The Examination of Colliery Ropes in Service. Reprint. Pp. 27. (London: H.M. Stationery Office, 1946.) 4*d.* net. [221

Water and Public Health. By W. Gordon Carey. (Twenty-eighth Streatfeild Memorial Lecture.) Pp. 24. (London: Royal Institute of Chemistry, 1946.) [221

Rothamsted Experimental Station, Harpenden. Report for the War Years 1939–1945. Pp. 269. (Harpenden: Rothamsted Experimental Station, 1946.) 6*s.* [221

Report upon the Needs of European Countries for Scientific Instruments and Laboratory Equipment. Pp. 109. (London: Ministry of Supply (Information Branch), 1946.) [231

Northumberland and Tyneside: a Bibliography. By W. C. Donkin and E. F. Patterson. (Published for the Ministry of Town and Country Planning.) Pp. xvi + 102. (London: H.M. Stationery Office, 1946.) 2*s.* net. [231

Other Countries

Indian Forest Leaflet No. 94: Classification of Silvicultural Ledger Files. Sixth revised edition. Pp. 36. (Dehra Dun: Forest Research Institute, 1946.) 6 annas; 7*d.* [141

The Mineral Resources of Western Australia. Prepared in the Government Mineralogical and Chemical Laboratories. Fifth edition. Pp. 28. (Perth: Government Printer, 1945.) [151

Indian Central Jute Committee. Technological Research Memoir No. 9: The Single Thread Strength of Jute Yarns in relation to Test Length. By K. R. Sen and C. R. Nodder. Pp. 20. 1 rupee; 1*s.* 6*d.* Technological Research Memoir No. 10: The Effect of Moisture on the Strength of Jute Yarns. By K. R. Sen, S. B. Bandyopadhyay and C. R. Nodder. Pp. 10. 6 annas; 6*d.* Technological Research Pamphlet No. 2: Assessment of Spinning Quality. By C. R. Nodder. Pp. 8. 6 annas; 6*d.* Technological Research Pamphlet No. 3: Some Notes on the Strength of Jute Ply-Yarns. By C. R. Nodder. Pp. 8. 6 annas; 6*d.* (Calcutta: Indian Central Jute Committee, 1945–1946.) [161

Anuario del Observatorio Astronómico de Madrid para 1947. Pp. 347. (Madrid: Instituto Geográfico, 1946.) [221

Bergens Museums Årbok. Naturvitenskapelig rekke, 1944. Heft 2, Nr. 7–Nr. 11. Pp. 238. (Bergen: A/s John Griegs Boktrykkeri, 1946.) [221

Annuaire astronomique de l'URSS pour l'an 1947. Vingt sixième année. (In Russian and French.) Pp. 452. (Moscow and Leningrad: Akademii Nauk SSR., 1946.) 75 roubles. [231

All-India Institute of Hygiene and Public Health. General Health Survey: Singur Health Centre. Preliminary Report, 1944. Pp. iii + 78. (Calcutta: All-India Institute of Hygiene and Public Health, 1946.) [231